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The Balanced Scorecard: Judgmental Effects of Performance Measures Linked to Strategy

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ABSTRACT: The balanced scorecard provides a framework for selecting multiple performance measures that supplement traditional financial measures with operating measures of customer satisfaction, internal processes, and learning and growth activities. An essential aspect of the balanced scorecard lies in its articulation of the linkage between performance measures and business strategy. This study conducts an experiment to assess how individuals' evaluations of the performance of business unit managers depend on strategically linked performance measures of a balanced scorecard. Statistical test results indicate that performance evaluations are influenced by strategically linked measures more than non-linked measures only when evaluators are provided detailed information about business unit strategies. The results also confirm Lipe and Salterio's (2000) finding that evaluators rely more on common measures than on unique measures. Evaluators rely more on strategically linked measures than on common measures when they are provided information on strategic linkages, but the reverse relation holds when they are not.

Keywords: *balanced scorecard; performance measures; strategic linkages; performance evaluation; business strategy.*

Data Availability: *Data are available from the authors upon request.*

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I. INTRODUCTION

The balanced scorecard (BSC) was introduced in 1992 to provide a framework for selecting multiple performance measures focused on critical aspects of business. The BSC also provides a tool for organizing strategic objectives into customer, internal process, and learning and growth perspectives, to augment the traditional financial perspective. By integrating the four perspectives, the BSC is expected to help managers understand cross-functional relationships that can ultimately lead to improved problem solving and decision making (Kaplan and Norton 1992). While the BSC was originally designed for multidimensional performance measurement, the concept has now evolved into an organizing framework for a strategic management system (Kaplan and Norton 1996).

An essential aspect of the BSC is the articulation of linkages between performance measures and strategic objectives (Banker et al. 2001). Once linkages are understood, strategic objectives can be further translated into actionable measures to help organizations improve performance (Kaplan and Norton 2000). In the accounting literature, several recent studies have followed this line of inquiry. For instance, Ittner and Larcker (1998a) describe trends in linking strategy to performance measurement; Banker et al. (2000) and Ittner and Larcker (1998b) provide empirical evidence for strategic linkages between financial and nonfinancial measures; and Krumwiede et al. (2000) provide experimental evidence on individuals' responses to BSCs developed for business units with different strategies. However, the issue of whether evaluators rely more on strategically linked measures than non-linked measures has not yet been examined.

In a recent study of the BSC, Lipe and Salterio (2000) designed an experiment to examine judgmental effects of common and unique measures on superiors' evaluation of business unit managers' performance. They found that evaluations are based only on BSC measures that are common across different business units; measures that are unique to individual business units are ignored. This result may be interpreted as undermining the BSC's major espoused benefit of uniquely capturing business strategy. However, while some of the common measures they examine are linked to a business unit's strategic objectives, some are not. This is also true of their unique measures. Since Lipe and Salterio (2000) did not categorize measures in terms of strategic linkages, their findings do not provide evidence on whether evaluators would rely on unique measures that are linked to a business unit's strategy.

To examine whether managers rely on strategically linked measures more than non-linked or common measures, we designed an experiment based on a case study of a clothing retailer operating several independent retail chains. Each chain is organized as a separate strategic business unit (SBU) with a distinct image and target market. We assigned M.B.A. students to evaluate the performance of two specific SBU managers, based on data presented in separate balanced scorecards for the two SBUs. We manipulated the reported performance of the two SBUs measured as actual relative to targets for linked, non-linked, common, and unique measures in a fully crossed design. Thus, one SBU may outperform the other SBU on common measures, unique measures, linked measures, non-linked measures, or a combination of them.

Since understanding of the SBU strategy is an essential precursor to reliance on strategically linked performance measures, we also manipulated the amount of strategic information available to experiment participants. We manipulated the detailed SBU strategy information at two levels: (1) no detailed SBU-specific information (benchmark treatment), and (2) narrative and graphical representations of SBU-specific strategies (strategy information treatment); expecting to find greater reliance on strategically linked measures in the latter case.[1]

Our statistical test results support these expectations. Consistent with Lipe and Salterio (2000), *ceteris paribus*, when asked to compare the performance of two SBUs, experiment participants rely significantly more on common measures. However, the availability of strategy information impacts participants' reliance on various measure types. In particular, when participants are provided with additional information on SBU strategies, strategically linked measures have a significantly greater impact on evaluations than non-linked measures. In this case, strategically linked unique measures also have a significantly greater impact on evaluations than common non-linked measures. Taken together, these findings suggest that when managers have an understanding of SBU strategy, linked measures dominate common measures in decision making; otherwise, common measures dominate linked measures.

We have organized this paper as follows. In Section II, we review related literature to motivate our research hypotheses. In Section III, we describe the design of our experiment. We present and discuss the results in Section IV. Finally, we discuss the implications of our findings in Section V.

II. DEVELOPMENT OF RESEARCH HYPOTHESES

Balanced Scorecard

Kaplan and Norton (1992) developed the BSC to supplement traditional financial measures with operating measures oriented toward customers, internal processes, and learning and growth activities. Financial measures, such as return on investment and return on sales, are used to indicate whether the company's strategy implementation and execution are contributing to improvements in the bottom line. Customer measures, such as customer satisfaction, are intended to measure the company's performance from the customer's perspective. Internal process measures, such as time to process customer returns in retail stores, are employed to identify core competencies, recognize strengths and shortcomings, and make improvements. Since the path to success for any business changes with time, a company's ability to innovate new products and new processes is critical in achieving excellence. Learning and growth measures, such as employee skills and computerization, focus on factors that facilitate continuous improvement.

Proponents of the BSC consider it imperative that measures used to evaluate performance be linked to the business strategy, regardless of whether they are measures common to all business units or unique to a particular unit (Kaplan and Norton 2000). The concept of strategically linked measures also underlies the growing emphasis on nonfinancial, forward-looking performance measures and value drivers in the performance measurement literature (Ittner and Larcker 1998b). Several contend that performance measurement systems, regardless of whether they use the BSC framework, should be designed to make cause-effect relations between managers' actions and results more explicit (e.g., Eccles 1991; Copeland et al. 1996; Young and O'Byrne 2001). Such systems enable employees at various levels of the organization to understand how their actions and performance measures translate into firm-wide performance. Strategic linkages also connect immediate measures, such as customer satisfaction, with long-term, lagging measures of performance like return on investment. Although the evidence is mixed, several empirical studies find positive relations between strategically linked performance measures and firm-wide, long-term financial performance (Anderson et al. 1994; Anderson et al. 1997; Banker et al. 1993; Banker et al. 2000; Ittner and Larcker 1996, 1998b).

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[1] Prior research indicates that individuals understand narrative information better when it is accompanied by graphical representations (Benbasat and Dexter 1986; Cohn and Cohn 1994).

While research on the performance effects of strategically linked measures supports their use, it does not directly address how managers' understanding of strategy influences the extent to which strategically linked performance measures impact managers' decisions. Kaplan and Norton (2000) emphasize that employees' understanding of strategy is critical to the success of the BSC. The better employees understand firm strategy, the better they will be able to use strategically linked performance measures to guide their decisions and actions. Krumwiede et al. (2000) examine this issue in an experiment designed to measure the impact of strategic linkages on managers' use of nonfinancial and financial performance measures. They find some evidence that managers rely more on nonfinancial measures that are linked to a firm's strategy; however, their results are not statistically robust.

Research Hypotheses

In order for managers to incorporate strategic linkages into performance evaluation, managers must understand SBU strategy. Implementations of the balanced scorecard now include graphical communication of the business strategy in the form of "strategy maps" that emphasize the linkages between various activities and performance measures (Kaplan and Norton 2000). Oliva et al. (1987) contend that strategy maps are instrumental in capturing the simultaneous, multidimensional, and interrelated nature of business strategy. Research on cognition suggests graphical representation makes it easier for individuals to identify relations among data, and instruction that combines verbal and graphical representation of concepts produces greater comprehension (Glenberg and Langston 1992; Kourilsky and Wittrock 1987; Wittrock 1974). Accordingly, we expect the perceived relevance of strategically linked measures, and consequently their use in performance evaluation, to be greater when managers are provided both verbal and graphical representation of SBU strategy (i.e., detailed strategy information) as compared to when managers are not provided such information. Furthermore, managers who have detailed strategy information will also recognize that non-linked measures are of little value and, therefore, they will rely less on them in evaluating performance than those who do not have detailed strategy information. This suggests the following hypothesis:

> **H1:** In evaluating performance, managers who have detailed strategy information will place greater (less) weight on strategically linked (non-linked) measures than those who have no detailed strategy information.

If managers do not have the detailed information necessary to understand business unit strategy, then we do not expect them to be predisposed to favoring either the strategically linked or non-linked measures. A consequence of H1 then is that managers will perceive the strategically linked performance measures to be more useful for performance evaluation than non-linked measures only when they have detailed strategy information. Therefore, we predict:

> **H2:** When managers have detailed strategy information, they will place more weight on strategically linked measures than they will on non-linked measures in evaluating performance. When managers do not have detailed strategy information, there will be no difference in the weights placed upon linked and non-linked measures.

Some strategically linked measures may be common to all business units and other measures may be unique to a specific business unit. Similarly, some non-linked measures may be common to all business units and some may not. Prior research has documented the tendency of experiment participants to rely more on common measures than on unique measures when comparing multiple subordinates (Slovic and MacPhillamy 1974; Lipe and Salterio 2000).

Comprehension of strategic linkages and recognition of the relevance of strategically linked measures is likely to be the key moderator determining the relative impact of common versus strategically linked measures. When managers understand the linkage between performance measures and business unit objectives, we expect that strategically linked measures will have a greater impact than common measures on performance evaluations. Conversely, when managers do not understand strategic linkages, they are more likely to rely on common measures. Since common strategically linked measures are included in both the set of common measures and the set of linked measures, this reduces to a comparison of unique linked measures to common non-linked measures. Accordingly, we hypothesize:

> **H3:** When managers have (do not have) detailed strategy information, they will place more (less) weight on unique linked measures than they will on common non-linked measures in evaluating performance.

III. DESIGN OF EXPERIMENT

Overview of Experiment

We replicated much of the structure of Lipe and Salterio's (2000) experiment to maximize comparability of our results. Participants in the experiment read a case asking them to assume the role of a senior executive at Smithson Stores, a clothing retailer that recently implemented a performance measurement system based on the balanced scorecard. The case focuses on the performance of two strategic business units (SBUs) of Smithson Stores: The Women's Store and The Family Store, each with a distinct strategy and target market. The case provides a narrative overview of the company, which includes Smithson's mission statement and a brief description of each SBU. The Women's Store is described as "a retailer of fashionable women's clothing...in the mature stage of the business lifecycle," and The Family Store is described as "a retailer catering to young families...in the growth stage of the lifecycle." The case also includes a balanced scorecard for each SBU that gives actual versus targeted performance for 16 performance measures, along with the definition of each performance measure (see Exhibit 1).

Participants were required to evaluate the performance of each SBU manager based upon the information provided in the case. Responses were elicited on a 13-point scale anchored as follows:

0 = Reassign: sufficient improvement unlikely;
2 = Very Poor: considerably below expectation, needs considerable improvement;
4 = Poor: somewhat below expectations;
6 = Average: meets expectations;
8 = Good: somewhat above expectations;
10 = Very Good: considerably above expectations; and
12 = Excellent: far beyond expectations, manager excels.

After rating each manager on the 13-point scale above, participants were asked to recommend one of the two managers for promotion to the position of a Vice President within

EXHIBIT 1
The Balanced Scorecard for The Women's Store Employed in the Experiment[a]

Measure	Target	Actual	Percent Better than Target
Financial			
1. Sales margins	60%	67.02%	11.70
2. Sales growth per store	15%	16.75%	11.67
3. Inventory turnover	6	6.59	9.83
4. Debt-to-assets ratio	<20%	18.07%	9.65
Customer			
1. Price relative to competitors' price	+7%	7.79%	11.29
2. Customer satisfaction rating	80%	88.44%	10.55
3. Sales per square foot of retail space	$30,000	$33,090	10.30
4. Number of credit card customers per store	8,000	8,911	11.39
Internal Process			
1. Brand recognition rating	80%	87.60%	9.50
2. Number of stock-outs	<3 times	2.66	11.33
3. "Mystery Shopper" audit rating	85%	93.47%	9.96
4. Time to process customer returns	<4 min.	3.54	11.50
Learning and Growth			
1. Employee satisfaction	80%	87.96%	9.95
2. Employee suggestions per year	2.5 times	2.74	9.60
3. Store computerization	60%	66.24%	10.40
4. Hrs. of training invested in brand managers per year	80 hours	89.10	11.38

[a] A similar scorecard was prepared for The Family Store, the other SBU in the experiment.

Smithson's. Finally, participants provided demographic information and answered questions regarding case comprehension, task difficulty, and realism.[2]

Experiment Participants

A total of 480 students enrolled in the M.B.A. core course in accounting completed the experiment in the one hour allotted for it. Analysis of the students' demographic background indicated that 49 percent were male, their average age was 29 years, 74 percent had lived in the U.S. for at least five years, and 96 percent had visited a retail clothing store in the past 12 months. Participants had an average of 6.4 years of full-time work experience, 19 percent had work experience in the retail industry, 13 percent had work experience in accounting, auditing, or taxation, and 11 percent had work experience in marketing or sales.

Manipulation of Performance Measures in Scorecard

We specified four performance measures for each of the four BSC perspectives, resulting in 16 measures for each SBU. Within each perspective, two measures are common to both SBUs and two measures are unique to each SBU (Lipe and Salterio 2000). In addition to the common/unique distinction used in Lipe and Salterio (2000), we also distinguish between measures that are linked to SBU strategy and those that are not. One common and one unique measure in each perspective are strategically linked and one common and one unique measure are non-linked. This results in four different combinations of performance measure types within each of the four BSC perspectives: common and linked (CL), common and non-linked (CN), unique and linked (UL), and unique and non-linked (UN). Exhibit 2 lists the performance measures for the two SBUs.

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[2] Responses to questions regarding case comprehension, task difficulty, and realism are discussed in the "Manipulation Checks" subsection.

For each performance measure, The Women's Store may outperform The Family Store or vice versa.[3] Since each of the four types (CL, CN, UL, and UN) of measures may either favor The Women's Store or The Family Store, there are 16 possible combinations of

EXHIBIT 2

Common, Unique, Linked and Non-Linked Performance Measures for The Women's Store and The Family Store Balanced Scorecards

Type[a]	Measures
Financial Perspective	
CL	Sales margins
CN	Debt-to-assets ratio
UL – W	Sales growth per store
UN – W	Inventory turnover
UL – F	Percentage of sales from new stores
UN – F	Operating cash flow as a percentage of sales
Customer Perspective	
CL	Customer satisfaction rating
CN	Price relative to competitors' price
UL – W	Sales per square foot of retail space
UN – W	Number of credit card customers per store
UL – F	Percentage of sales to new customers
UN – F	Number of new items in which first to market
Internal Process Perspective	
CL	"Mystery Shopper" audit rating
CN	Time to process customer returns
UL – W	Brand recognition rating
UN – W	Number of stock-outs
UL – F	Market share per advertising dollar
UN – F	Vendor rating
Learning and Growth Perspective	
CL	Employee satisfaction
CN	Store computerization
UL – W	Hours of training invested per brand manager per year
UN – W	Employee suggestions per year
UL – F	Retail experience of marketing managers
UN – F	Sales associates with college degrees

[a] CL = a common linked measure;
CN = a common non-linked measure;
UL – W = a unique linked measure for The Women's Store;
UN – W = a unique non-linked measure for The Women's Store;
UL – F = a unique linked measure for The Family Store; and
UN – F = a unique non-linked measure for The Family Store.

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[3] As in Lipe and Salterio (2000), we required that the four performance measures of the same type favor the same SBU. For example, if a participant receives a scorecard in which The Women's Store dominates on common non-linked measures (CN), then the scores for the common non-linked measure in *each* perspective of the scorecard (financial, operational, customer, growth, and learning) will exceed those of The Family Store.

favored SBU and performance measure type, as shown on the left-hand side of Exhibit 3. Thus, with regard to the performance measures, we employed a 2 × 2 × 2 × 2 between-subjects experimental design.[4] As each participant evaluated both SBU managers, there is also a within-subjects factor.

We develop 16 different scorecard versions to reflect each of the 16 favored SBU/performance measure type combinations. For example in scorecard version 1 (Exhibit 3), The Women's Store outperforms The Family Store on all four measure types; in version 2, The Women's Store scores higher on CL, CN, and UL measures, but lower on UN measures; in version 3, The Women's Store scores higher on CL, CN, and UN measures, but lower on UL measures; etc. We assigned 30 participants to each of 16 different groups corresponding to the 16 different scorecard versions. *Ceteris paribus*, comparisons of performance evaluations across certain combinations of groups can potentially identify differences in the extent to which measure type affects evaluations.

We manipulated all numerical performance measure data as in Lipe and Salterio (2000). We chose actual performance measure values such that each SBU performed better than its target for all measures on the scorecard. However, the amount by which each SBU exceeded target varied by scorecard version to accommodate the 16 different favored SBU/performance measure combinations. Each measure type had approximately the same excess performance (percentage above target). The average percentage above target was between 10.5 percent and 10.7 percent for the better-performing SBU and between 6.4 percent and 6.6 percent for the worse-performing SBU for each of the four types (CL, CN, UL, and UN) of performance measures.[5]

Manipulation of Information Provided to Participants

To assess whether information about business unit strategy affects performance evaluations, we created two treatments: a benchmark treatment (Treatment B) and a strategy information treatment (Treatment S). Depending upon the treatment to which a participant had been assigned, we provided additional materials describing each SBU's strategy. Participants assigned to the benchmark treatment received a narrative overview of the company and two of its SBUs. Participants assigned to the strategy information treatment received a brief narrative and a simple strategy map (Kaplan and Norton 2000) outlining each individual SBU's strategy (see Exhibit 4) in addition to the narrative overview provided to participants in the benchmark treatment. In any organization utilizing the BSC, a precondition is that managers be made aware of the organization's strategy. If managers do not understand the strategy, then the BSC may not be useful. Therefore, in designing the experiment, we provided participants in the strategy information treatment with explicit and salient information about the SBU strategies. As participants in the benchmark treatment had no detailed information on SBU strategy, they presumably had no firm basis to identify strategically linked measures. Conversely, those in the strategy information treatment could comprehend strategy and, hence, discern the strategic significance of linked measures. Therefore, comparisons across treatments (but within the 16 scorecard types) provide evidence on whether strategy information affects performance evaluation.

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[4] The first factor indicates which SBU was favored on CL measures, the second factor indicates which SBU was favored on CN measures, the third factor indicates which SBU was favored on UL measures, and the fourth factor indicates which SBU was favored on UN measures. Full crossing between the four factors yields 16 groups.

[5] Due to rounding, the exact percentage varied slightly across various measures.

EXHIBIT 3
Average of Ratings Differences for Each Scorecard Version-Treatment Group

$$\Delta - RATINGS_{VERSION,TREATMENT} = [\Sigma_{i=1}^{15} (W_i - F_i)]/15$$

where W_i and F_i are participant i's evaluations of The Women's Store and The Family Store managers, respectively; and participant i received the scorecard VERSION and TREATMENT indicated.[a]

Scorecard Version	Types of Measures That Favor		Benchmark Treatment[b]	Strategy Information Treatment[b]	Difference between Treatments[c]
	The Women's Store	The Family Store	$\Delta - RATINGS_{VERSION,B}$	$\Delta - RATINGS_{VERSION,S}$	$\Delta - RATINGS_{VERSION,B} - \Delta - RATINGS_{VERSION,S}$
1	All	None	1.333***	1.400***	−0.067
2	CL, CN, UL	UN	1.066***	1.333***	−0.267
3	CL, CN, UN	UL	0.867	1.200**	−0.333
4	CL, CN	UL, UN	0.800**	0.667**	0.133
5	CL, UL, UN	CN	0.800**	1.400***	−0.600
6	CL, UL	CN, UN	0.267	0.733*	−0.467
7	CL, UN	CN, UL	0.133	−0.133	0.267
8	CL	CN, UL, UN	0.200	0.400	−0.200
9	CN, UL, UN	CL	0.800	0.467	0.333
10	CN, UL	CL, UN	0.333	0.200	0.133
11	CN, UN	CL, UL	−0.067	−0.867**	0.800
12	CN	CL, UL, UN	−0.533	−0.733*	0.200
13	UL, UN	CL, CN	−0.267	−0.067	−0.200
14	UL	CL, CN, UN	−1.133***	−0.533	−0.600
15	UN	CL, CN, UL	−0.600*	−0.800**	0.200
16	None	All	−1.133***	−1.000***	0.133

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[a] *, **, *** indicate significance at the 10 percent, 5 percent, and 1 percent levels, two-tailed, respectively.

[b] Significance levels refer to means test for paired data to evaluate the null hypothesis that Rating for The Women's Store = Rating for The Family Store.

[c] A means test for two independent samples was used to evaluate the null hypothesis that $\Delta - RATINGS_{VERSION,B} = \Delta - RATINGS_{VERSION,S}$. This null hypothesis cannot be rejected at a significance level of at least 10 percent (two-tailed) for any of the scorecard version groups.

EXHIBIT 4
Strategy of Two Strategic Business Units of Smithson Stores
(provided in the strategy information treatment)

Presented below is a brief description of the strategy of The Women's Store and The Family Store.

The Women's Store

The Women's Store (TWS) is an established specialty retailer that caters to fashion-conscious professional women. TWS's strong cash flows will be needed to fuel growth in Smithson's younger SBUs. TWS will contribute to corporate objectives by using its existing store network to further penetrate its target market, while improving margins and cash flows. Accordingly, sales growth will come through the introduction of new clothing lines, such as its business casual line, and excellent in-store shopping assistance designed to accommodate style-conscious, time-constrained customers. TWS will leverage its distinctive brand image to drive new clothing sales and margin growth within its target market, which is not very price-sensitive. TWS's goal is to become a store in which women can shop for all of their wardrobe needs, from clothing to accessories to shoes, in one convenient location. By developing skilled brand managers who can broaden its product line to accommodate "one-stop shopping" and by offering greater in-store shopping assistance, TWS hopes to compete with catalogue and online retailers. Central to TWS's growth strategy is the creation of a "perfect in-store shopping experience" that will entice busy women to visit the store, rather than shop online or via the phone.

The Family Store

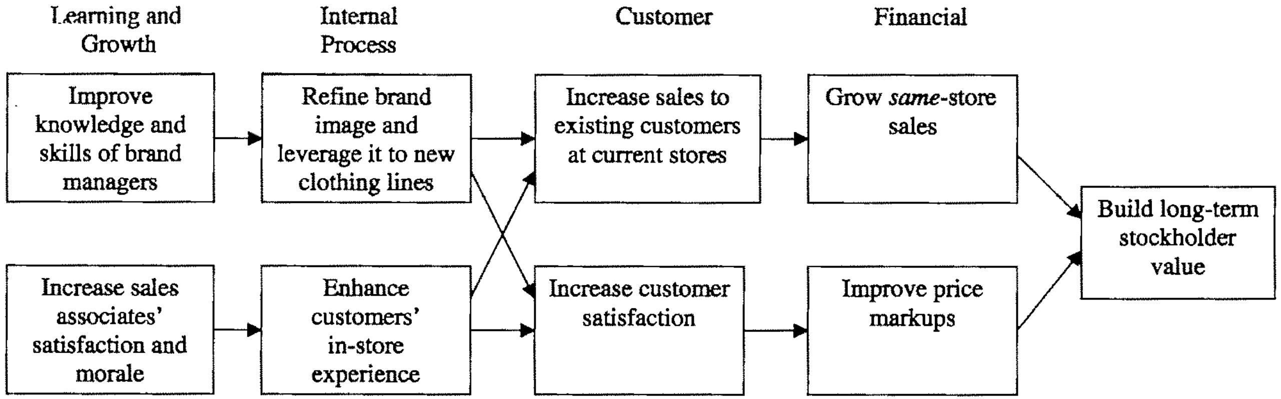
The Family Store (TFS), which opened its doors three years ago, is one of several retail chains in the Smithson portfolio that is still in the "growth" stage of the business lifecycle. TFS carries classic, high-quality casual clothing, such as khakis, jeans, and polo shirts for men, women, and children. Although TFS offers stylish, high-quality merchandise, it is more of "fashion follower" than a "fashion leader." Therefore, unlike The Women's Store, which seeks to introduce many new product lines, TFS identifies a relatively narrow set of basic, functional styles each season and offers these items in a variety of colors and sizes. Management expects this new, high-end retailer to drive a significant portion of Smithson's overall growth, as TFS plans to double the number of stores over the next two years.

TFS's target customers are primarily young, upper middle-class families with significant disposable income, but little free time. TFS's combination of classic, high-quality men's, women's, and children's apparel enables busy parents to shop for the entire family in one location. To attract this customer segment to its store, TFS is building an experienced marketing team that will launch an aggressive advertising campaign featuring memorable, humorous commercials appealing to all ages. Additionally, TFS plans to exceed customers' shopping expectations with its fun "in-store" atmosphere, visually appealing displays, and excellent service.

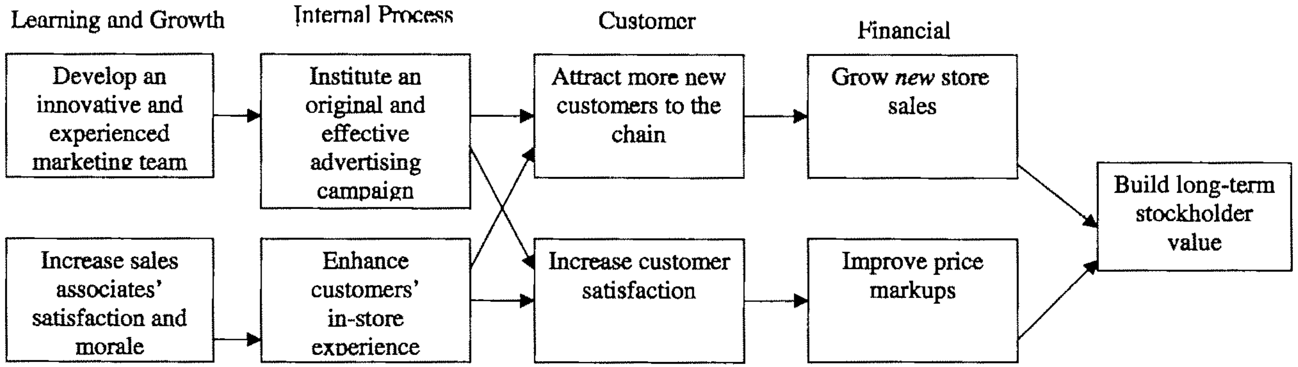
(continued on next page)

EXHIBIT 4 (continued)

The Women's Store: Strategic Objectives


Learning and Growth
Internal Process
Customer
Financial
Improve knowledge and skills of brand managers
Refine brand image and leverage it to new clothing lines
Increase sales to existing customers at current stores
Grow *same*-store sales
Build long-term stockholder value
Increase sales associates' satisfaction and morale
Enhance customers' in-store experience
Increase customer satisfaction
Improve price markups

The Family Store: Strategic Objectives


Learning and Growth
Internal Process
Customer
Financial
Develop an innovative and experienced marketing team
Institute an original and effective advertising campaign
Attract more new customers to the chain
Grow *new* store sales
Build long-term stockholder value
Increase sales associates' satisfaction and morale
Enhance customers' in-store experience
Increase customer satisfaction
Improve price markups

Recall that there were 30 participants for each of the 16 different scorecard types. Half of the 30 participants within each of the 16 groups were assigned to the benchmark treatment and the other half were assigned to the strategy information treatment. This resulted in 15 participants in each of 32 different groups corresponding to the 16 scorecard versions and the two information treatments. In summary, the experiment has a 2^6 mixed design.

Pilot Test

We adapted the instrument developed by Lipe and Salterio (2000) for our experiment. The adapted instrument was reviewed by six academics with expertise in management accounting. We conducted an additional pilot test using 45 Executive M.B.A. students as participants. The pilot participants rated their agreement (on an 11-point scale where −5 indicates "strongly disagree" and +5 indicates "strongly agree") with statements assessing the usefulness of performance measure categorizations, the understandability of the case, and the realism of the case. Mean agreement ratings were statistically significantly greater than zero for each question (all $p < 0.001$, two-tailed). This suggests the case was reasonable and easy to understand. Further analysis of participants' responses indicated no significant *a priori* difference between the perceived relevance of the set of strategically linked and the set of non-linked measures for retail stores.

Dependent Measures

Each participant rated the performance of The Women's Store manager and The Family Store manager on a 13-point scale. The variable of interest for the hypothesis tests is the difference between the evaluation scores the same participant assigned to each SBU manager. This difference is denoted as $(W_i - F_i)_{VERSION,TREATMENT}$, where W_i and F_i are participant i's rating for The Women's Store and The Family Store, respectively; and participant i received the scorecard *VERSION* (one of 16) and *TREATMENT* (one of 2) indicated. Also of interest is the average of the ratings differences given by the 15 participants in each of the 32 scorecard version-treatment groups, denoted $\Delta - RATINGS_{VERSION,TREATMENT}$ (Equation (1)).

$$\Delta - RATINGS_{VERSION,TREATMENT} = [\Sigma_{i=1}^{15} (W_i - F_i)]/15 \qquad (1)$$

where W_i and F_i are participant i's ratings for The Women's Store and The Family Store, respectively, and participant i received the scorecard *VERSION* and *TREATMENT* indicated. The denominator is 15 because there are 15 participants in each scorecard version-treatment group. For example, $\Delta - RATINGS_{1,B}$ is the average ratings difference reported by the 15 participants in the benchmark treatment that received scorecard version 1 described in Exhibit 3. As shown in the first row of Exhibit 3, the value of $\Delta - RATINGS_{1,B}$ is 1.333. This indicates that participants in the benchmark treatment who received scorecard version 1 assigned The Women's Store manager a rating that was, on average, higher than that assigned to The Family Store manager by 1.333. If $\Delta - RATINGS_{VERSION,TREATMENT}$ is negative, then the ratings for The Family Store Manager were higher, on average, than those for The Women's Store.

IV. RESULTS

Manipulation Checks

An important premise underlying our theoretical arguments is that narrative information accompanied by graphical representation leads to a better understanding of strategy, which in turn leads to greater reliance on strategically linked measures. To test this premise, we developed a measure of strategy comprehension from four survey questions. The statements were designed to capture the essential elements of each SBU's strategy (see Panel A, Exhibit 5). After reading the materials and evaluating each SBU manager, we asked experiment participants to rate their agreement with each strategy statement using an 11-point scale ranging from −5 (strongly disagree) to +5 (strongly agree). To confirm that the statements measure a single construct, we factor analyzed the four responses. The responses loaded on a common factor that we labeled "*strategy comprehension*" and had a Cronbach's coefficient alpha of 0.60, thus indicating the statements were internally consistent. We

EXHIBIT 5

Mean Responses to Questions regarding Participants' Strategy Comprehension and Understanding of Task by Treatment[a]

	Benchmark Treatment[b]	Strategic Treatment[b]	Difference in Treatment Means[c]
Panel A: Strategy Comprehension			
1. The strategy of The Women's Store is to generate greater sales through its *existing* infrastructure rather than invest in new stores.	3.23	3.73	−0.50**
2. To grow sales, The Women's store must successfully introduce new lines of clothing to its existing customers.	2.48	3.38	−0.90**
3. The strategy of The Family Store is to grow by adding new stores.	2.99	3.54	−0.55**
4. The Family Store needs an innovative marketing group because its growth plans depend upon the success of its advertising campaign in attracting new customers.	2.96	3.67	−0.71**
Strategy comprehension scale[d]	2.91	3.58	−0.67***
Panel B: Understanding of Task			
1. The performance measures were usefully categorized in this case.	2.64	2.80	−0.16
2. The emphasis on financial measures was appropriate.	1.94	2.21	−0.27
3. The two business units, The Women's Store and The Family Store, used some different performance measures.	3.15	3.50	−0.35*
4. It was appropriate for the two business units, The Women's Store and The Family Store, to employ the different performance measures.	3.33	3.90	−0.57**
5. The case was easy to understand.	2.40	2.90	−0.50**
6. The case was difficult to do.	−1.02	−1.04	0.02
7. The case was realistic.	2.68	3.21	−0.53**

[a] Students were asked to rate their agreement with each statement on an 11-point scale where −5 was labeled "strong disagree" and +5 was labeled "strongly agree."

[b] Mean responses for all participants significantly exceed 0 ($p < 0.01$, one-tailed); except for question 6 in Panel B, where, mean response for all participants is significantly less than 0 ($p < 0.05$, one-tailed).

[c] A two-sample means test was used to test the null hypothesis that means for both treatments are equal. * and ** indicate that the null hypothesis can be rejected at significance levels of $p < 0.05$ and $p < 0.01$, respectively.

[d] Average of responses to questions 1–4 in Panel A.

developed a numerical measure of *strategy comprehension* by taking an average of the four responses. The mean value of *strategy comprehension* was statistically significantly greater ($t = 6.73$, $p < 0.01$, one-tailed) for the strategy information treatment (mean = 3.58) than for the benchmark treatment (mean = 2.91), suggesting that participants who were provided with strategy information did, in fact, have a better understanding of strategy than those in the benchmark treatment.

We also compared strategy comprehension across treatments after controlling for demographic variables that may affect participants' understanding of strategy, such as age, gender, U.S. residency, and work experience. We performed this comparison, by regressing *strategy comprehension* on the demographic variables and an indicator variable for the treatment. Although strategy comprehension was statistically significantly lower for participants who had lived in the U.S. for 5 or fewer years ($t = 2.60$, $p < 0.01$, two-tailed), participants in the strategy information treatment still showed statistically significantly greater comprehension of business strategy ($t = 6.06$, $p < 0.01$, one-tailed).[6]

Finally, we asked participants to rate on an 11-point scale their agreement with statements assessing the overall understandability of the case (Panel B, Exhibit 5). The statements addressed whether it was "appropriate for the two business units to employ some different performance measures" and whether the case was "easy to understand" and "realistic." Agreement ratings for each of these statements were statistically significantly greater ($t = 3.89$, 3.18, and 3.59, respectively; all $p < 0.01$, two-tailed) for those in the strategy information treatment than those in the benchmark treatment. This is consistent with the premise that SBU-specific narrative and graphical information lead to greater strategy comprehension. Moreover, the mean scores for all participants, regardless of treatment, for each of the aforementioned statements were statistically significantly greater than zero (t-statistics range from 19.6 to 36.0, all $p < 0.01$, one-tailed), indicating general agreement. This suggests that the participants had an adequate understanding of the case and felt it was realistic.

Statistical Methods

We randomly assigned each of the 480 participants to one of 32 groups, corresponding to the 32 unique combinations of scorecard version and information treatment described in Section III. To facilitate a preliminary visual comparison, Exhibit 3 reports the average of the difference in evaluation scores between the two SBU managers, $\Delta - \mathit{RATINGS}_{\mathit{VERSION,TREATMENT}}$, for each of the 32 groups. To investigate the hypotheses, we compare average ratings differences across various combinations of scorecard version-treatment groups using two different statistical methods. First, we employ means tests, which provide simple, one-dimensional, cross-sectional comparisons that are easy to interpret and have directional significance. One drawback of means tests is that they do not control for factors outside of

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[6] Significance level corresponds to the test of whether the coefficient on *Treatment S* differs from 0 in the following regression equation where *Treatment* S_i equals 1 if participant i was in the strategy information treatment and 0 otherwise:

$$\begin{aligned}\mathit{Strategy\ Comprehension}_i = {} & \alpha + \beta_1\, \mathit{Treatment\ S}_i + \gamma_1\, \mathit{Male}_i + \gamma_2\, \mathit{Age}_i + \gamma_3\, \mathit{Work\ Experience}_i \\ & + \gamma_4\, \mathit{Retail\ Experience}_i + \gamma_5\, \mathit{Years\ in\ US{<}5}_i \\ & + \gamma_6\, \mathit{Number\ of\ Retail\ Visits}_i + \gamma_7\, \mathit{WorkExp\text{-}A\&A}_i \\ & + \gamma_8\, \mathit{WorkExp\text{-}M\&S}_i + \gamma_9\, \mathit{Major\text{-}A\&A}_i + \gamma_{10}\, \mathit{Major\text{-}M\&S}_i + \varepsilon_i.\end{aligned}$$

See notes to Exhibit 7 for definition of terms.

the single cross-section of interest. To overcome this limitation, we also estimate a regression that incorporates various demographic variables.[7]

Means Tests

Common versus Unique

As the tendency for evaluators to rely more on common than unique measures is relevant to our research question, we first test whether this finding holds in our data (Slovic and MacPhillamy 1974; Lipe and Salterio 2000). We compare, within treatments, the impact that common and unique measures have on evaluations. To determine the impact of common measures, we contrast the ratings differences, $W_i - F_i$, for scorecard versions in which The Women's Store is favored on *all* common measures (versions 1–4, Exhibit 3) with the ratings differences for scorecard versions in which The Women's Store is *not* favored on *any* common measures (versions 13–16, Exhibit 3). In the benchmark treatment, the mean of the ratings differences for versions in which The Women's Store is favored on all common measures is 1.017 ({1.333 + 1.066 + 0.867 + 0.800}/4, Exhibit 3); and, the mean for versions in which The Women's Store is *not* favored on *any* common measures is −0.783 ({−0.267 − 1.133 − 0.600 − 1.133}/4, Exhibit 3). In Exhibit 6 (Panel A), we calculate the mean impact of common measures by subtracting the mean ratings difference for versions in which The Women's Store is *not* favored on *any* common measures from the mean ratings difference for versions in which The Women's Store is favored on all common measures (1.017 − {−0.783} = 1.800).[8]

Similarly, to determine the mean impact of unique measures, we subtract the mean of the ratings differences for scorecard versions in which The Women's Store is *not* favored on *any* unique measures (versions 4, 8, 12, and 16) from the corresponding mean for scorecard versions in which The Women's Store is favored on *all* unique measures (versions 1, 5, 8, and 13). This difference, 0.833 (0.667 − {−0.166}), is compared to the mean impact of common measures, 1.800, using a one-tailed two-sample means test (means provided in Exhibit 6). Consistent with the findings of Slovic and MacPhillamy (1974) and Lipe and Salterio (2000), the ratings differences for common measures statistically significantly exceed those for unique measures in the benchmark ($t = 2.94$, $p < 0.01$) and strategy information treatments ($t = 2.12$, $p = 0.02$). Participants rely more on common measures than unique measures in evaluating performance.

Since both common and unique measures can be linked or non-linked, we also analyze linked common and unique measures separately from non-linked common and unique measures. We compare, within treatments, the impact of common linked measures to the impact of unique linked measures. Consistent with the prior result, the mean impact of common linked measures statistically significantly exceeds the mean impact for unique linked measures in the benchmark ($t = 2.42$, $p < 0.01$) and strategy information ($t = 2.00$, $p = 0.02$)

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[7] We also used ANOVA to evaluate the data. However, the presence of several significant interaction terms makes the direct interpretation of ANOVA complex. As ANOVA and multiple regression are based upon the same statistical model, results of the regression (without inclusion of demographic variables) are consistent with those of ANOVA (Keppel and Zedeck 1989, Chapter 25). Without the demographic variables, the regression results are also identical to the means tests.

[8] Since the experiment is symmetric, i.e., The Family Store is favored on all measures on which The Women's Store is not favored, the results will be the same if we replace The Women's Store with The Family Store in this and subsequent calculations. For example, The Family Store is favored on *all* common measures (i.e., The Women's Store is favored on *no* common measures) in versions 13–16 and the corresponding mean of the ratings differences is −0.783. The Family Store is *not* favored on *any* common measures (i.e., The Women's Store is favored on all common measures) in versions 1–4, and the mean of the ratings differences is 1.017. Thus, the impact of common measures is the absolute value of the difference in these two means ($| -0.783 - 1.017 | = 1.800$), albeit with directional significance.

EXHIBIT 6
Comparison of Mean Impact of Various Types of Measures

	Benchmark Treatment			Strategy Information Treatment			
	Mean of ratings differences for versions in which The Women's Store is favored on all (none) of the measure type indicated[a]			Mean of ratings differences for versions in which The Women's Store is favored on all (none) of the measure type indicated[a]			
Types of Measures	(1) All Favor The Women's Store	(2) None Favors The Women's Store	Mean Impact[b] (3) = (1) − (2)	(4) All Favor The Women's Store	(5) None Favors The Women's Store	Mean Impact[b] (6) = (4) − (5)	H1: (t-statistic) Significance for Across-Treatment Comparison of Mean Impacts: (3) versus (6)[c]
Panel A: Lipe and Salterio							
Common (C)	1.017	−0.783	1.800	1.150	−0.600	1.750	(0.19) n.s.
Unique (U)	0.667	−0.166	0.833	0.800	−0.166	0.966	(0.29) n.s.
(t-statistic), significance[d]			(2.94) 0.00			(2.12) 0.02	
Common, Linked (CL)	0.683	−0.325	1.008	0.875	−0.416	1.291	
Unique, Linked (UL)	0.400	−0.042	0.442	0.617	−0.158	0.775	
(t-statistic), significance[d]			(2.42) 0.00			(2.00) 0.02	
Common, Non-Linked (CN)	0.575	−0.216	0.791	0.458	0.000	0.458	
Unique, Non-Linked (UN)	0.375	−0.016	0.391	0.325	0.133	0.192	
(t-statistic), significance[d]			(1.69) 0.05			(1.01) 0.15	
Panel B: H2							
Linked (L)	0.867	−0.583	1.450	1.216	−0.850	2.066	(1.91) 0.03
Non-Linked (NL)	0.733	−0.450	1.183	0.550	0.000	0.650	(1.43) 0.08
(t-statistic), significance[d]			(0.77) n.s.			(4.00) 0.00	
Common, Linked (CL)	0.683	−0.325	1.008	0.875	−0.416	1.291	
Common, Non-LINKED (CN)	0.575	−0.216	0.791	0.458	0.000	0.458	
(t-statistic), significance[d]			(0.89) n.s.			(3.10) 0.00	
Unique, Linked (UL)	0.400	−0.041	0.441	0.617	−0.158	0.775	
Unique, Non-Linked (UN)	0.375	−0.016	0.391	0.325	0.133	0.192	
(t-statistic), significance[d]			(0.22) n.s.			(2.38) 0.01	

(continued on next page)

EXHIBIT 6 (continued)

Panel C: H3

Unique, Linked (UL)	0.400	−0.041	0.441	0.617	−0.158	0.775
Common, Non-Linked (CN)	0.575	−0.216	0.791	0.459	0.000	0.458
(t-statistic), significance[d]			(1.50) 0.07			(1.23) 0.11

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[a] All of a measure type favoring The Women's Store = None of a measure type favoring The Family Store. For example, if The Women's Store is favored on all common measures in a scorecard, then The Family Store is not favored on any common measures in that version.

[b] The Mean Impact of measures is calculated by subtracting ratings differences for scorecard versions in which The Women's Store is favored on no measures of the type indicated from ratings differences for versions in which The Women's Store is favored on all measures of the type indicated (C, U, CL, UL, CN, UN).

[c] t-statistics are for across-treatment comparisons of Mean Impact of measures, i.e., comparisons of columns (3) and (6). Significance levels in Panel A (Common versus Unique) are two-tailed as no prediction was made regarding across-treatment variation in these measures. Significance levels in Panel B (Linked vs. Non-Linked) are one-tailed, consistent with H1.

[d] t-statistics are in parentheses below Mean Impacts being compared in means tests and significance levels are next to t-statistics. In Panel A, means tests are one-tailed as we expect the differences to be greater for common measures than for unique measures. In Panel B, means tests are two-tailed in the benchmark treatment and one-tailed in the strategy information treatment, consistent with the predictions of H2. In Panel C, means tests are one-tailed, consistent with H3.

treatments. When we compare the mean impact of common non-linked measures to unique non-linked measures, the result holds in the benchmark treatment ($t = 1.68$, $p = 0.05$), but is not significant in the strategy information treatment ($t = 1.04$, $p = 0.15$). One potential explanation for this finding may be that participants with detailed strategy information disregard non-linked measures, regardless of whether they are common. Overall, the largest contrasts between ratings differences occur in the benchmark treatment, which suggests that participants rely more on common measures in the absence of detailed strategy information.

Impact of Strategy Information

Our first hypothesis considers how detailed information on SBU strategy affects managers' reliance on different types of measures when evaluating SBU managers. We test this hypothesis by comparing the impact of various measures across treatments using a one-tailed, two-sample means test. To evaluate the influence of strategy information on participants' use of linked measures, we first calculate the impact of linked and non-linked measures within each treatment, as we did in the previous section (see Exhibit 6). Results are consistent with H1. The impact of linked measures is statistically significantly greater ($t = 1.91$, $p = 0.03$) in the strategy information treatment (2.066) than in the benchmark treatment (1.450).[9] Conversely, the impact of non-linked measures is marginally statistically significantly greater ($t = 1.43$, $p < 0.08$) in the benchmark treatment (1.183) than in the strategy information treatment (0.650). The results indicate that participants who are provided detailed strategy information perceive linked measures to be more useful and non-linked measures to be less useful than participants who are not provided detailed strategy information.

Linked versus Non-Linked Measures

Hypothesis 2 predicts that managers rely more on strategically linked measures than non-linked measures when they are provided detailed information on business unit strategy. The test of this hypothesis is analogous to the test used to compare participants' reliance on common and unique measures. Within each treatment, we compare the impact of linked measures to the impact of non-linked measures (Panel B, Exhibit 6). The mean impact of linked measures, 1.450, does not significantly differ from the mean impact of non-linked measures, 1.183, in the benchmark treatment ($t = 0.77$, n.s.). In the absence of detailed strategy information, participants do not rely differentially on linked and non-linked measures. This result also provides verification that participants are not predisposed to our strategically linked measures over our non-linked measures. In contrast to the results for the benchmark treatment, the mean impact of linked measures, 2.066, statistically significantly exceeds the mean impact of non-linked measures, 0.650, in the strategy information treatment ($t = 4.00$, $p < 0.01$). Taken together, the results for the benchmark and strategy information treatments are consistent with H2. Participants without detailed strategy information do not distinguish between linked and non-linked measures; participants with detailed strategy information place more weight on strategically linked measures than on non-linked measures.

These results hold when we consider whether linked and non-linked measures are common or unique. That is, in the strategy information treatment, the impact of common

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[9] We evaluate all our statistical tests at the 10 percent significance level, one-sided only when we have a directional prediction. In order to maintain consistency with terms used in the behavioral accounting literature, we describe results that are significant for a test at the 10 percent level but not significant for a similar test at the 5 percent level as "marginally statistically significant."

(unique) linked measures statistically significantly exceeds that of common (unique) non-linked measures ($t = 3.10$, $p < 0.01$, common; $t = 2.38$, $p = 0.01$, unique). However, in the benchmark treatment, common and unique measures that are linked do not differ statistically significantly from common and unique measures that are non-linked. Interestingly, while the results for unique linked measures in the benchmark treatment replicate those reported by Lipe and Salterio (2000), the corresponding results in the strategic information treatment are inconsistent with those of Lipe and Salterio, who find that unique measures have no effect on evaluations. We attribute this inconsistency to the difference in the information set available to participants. In our experiment, participants in the strategy information treatment received longer, more detailed narrative descriptions of SBU strategies than participants in Lipe and Salterio's (2000) experiment (see Exhibit 2).[10] Participants in our strategy information treatment also received a diagram of the SBUs' strategies. The explicit nature of our SBU strategy descriptions potentially made it easier for participants to understand SBU strategies and apply that understanding to identify and use linked measures, regardless of whether the measures were unique or common.[11] The strategy descriptions used by Lipe and Salterio (2000) are closer to those used in our benchmark treatment.

Linked versus Common Measures

Our final hypothesis posits that managers who have detailed strategy information will rely more on unique linked measures than common non-linked measures, while those who do not have detailed strategy information, will rely more on common non-linked measures than unique linked measures. We compare, within treatments, the mean impact of unique linked measures to that of common non-linked measures using a one-tailed two-sample means test (Panel C, Exhibit 6). The results are consistent with our hypothesis. In the strategy information treatment, the mean impact of unique linked measures, 0.775, marginally statistically significantly ($t = 1.50$, $p = 0.07$) exceeds the impact of common non-linked measures, 0.458. In the benchmark treatment, the impact of common non-linked measures, 0.791, exceeds that of unique linked measure, 0.441, although, the difference is not statistically significant ($t = 1.23$, $p = 0.11$) at the 10 percent level.

Regression Analysis

Random assignment of the 480 participants to the 32 combinations of scorecard versions and treatments reduces the likelihood that systematic differences in participants will confound the results. However, since the random assignment may not fully control for individual differences such as education, work experience, and gender, we use a regression model to incorporate these factors into our estimations.[12] As with the means tests, the dependent variable in the regression model, $W_i - F_i$, is the difference between participant i's ratings for the two SBU managers. This difference, $W_i - F_i$, is regressed on indicator variables that capture the four performance measure types (CL, CN, UL, UN), the two treatments (B, S), all possible higher-order interactions between measure type and treatment, and a set of control variables that measure participant-specific characteristics. An F-test of the model rejects the null hypothesis that all of the coefficients on the control variables equal zero ($F = 3.71$, $p < 0.05$). In particular, a control variable measuring familiarity with retail clothing stores is positively and statistically significantly ($t = 3.35$, $p < 0.01$, two-tailed) associated with the ratings difference. Control variables measuring age ($t = 1.71$, $p = 0.09$, two-tailed) and experience in Marketing or Sales ($t = 1.62$, $p = 0.11$, two-tailed) also have some impact on participants' ratings for the two managers. Hence, including the control variables in the analysis should yield more precise estimates of the impact of measure type and treatment on performance evaluation.

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[10] The strategy descriptions in our strategy information treatment totaled 415 words and discussed specific objectives relevant to each of the BSC perspectives. Strategy descriptions in Lipe and Salterio (2000) totaled 146 words and stated SBU objectives in broad terms.

[11] As discussed in the "Manipulation Checks" subsection, participants in the strategy information treatment showed significantly greater comprehension of SBU strategy and significantly more agreement with the statement, "It was appropriate for the two SBUs...to use some different measures," than participants in the benchmark treatment (Exhibit 5). This suggests that the strategy information treatment was much more effective in conveying the strategies to participants in a brief time, resulting in the participants' appreciation of the need to have SBU-specific performance measures.

[12] Recall from the "Manipulation Checks" subsection that participants who were not U.S. born and had spent less than five years in the U.S. exhibited significantly lower strategy comprehension than the remainder of the participants.

We estimate the mean impact of measure types and treatments by taking the difference between the estimated value of the regression expression in Exhibit 7 when the variable of interest equals 1 and when it equals 0, calculated at the mean values of other variables (e.g., Govindarajan and Gupta 1985).[13] Exhibit 7 contains the estimated mean impact of various measure types calculated from the regression analysis. The results confirm our findings from the means tests for the first two hypotheses and now provide stronger support for the third hypothesis. Consistent with the findings of Lipe and Salterio (2000) and Slovic and MacPhillamy (1974), common measures (1.872 for benchmark; 1.726 for strategy information) have a statistically significantly greater impact on performance evaluations than unique measures (0.802 for benchmark, 0.999 for strategy information) in both the benchmark ($t = 2.91$, $p = 0.01$, one-tailed) and the strategy information ($t = 2.00$, $p = 0.02$, one-tailed) treatments (Panel A, Exhibit 7). As predicted by the first hypothesis, the estimated mean impact of linked measures is marginally statistically significantly greater ($t = 1.48$, $p = 0.07$, one-tailed) for participants in the strategy information treatment (2.071) than it is for those in the benchmark treatment (1.527). Correspondingly, the mean impact of non-linked measures is marginally statistically significantly greater ($t = 1.36$, $p = 0.09$, one-tailed) in the benchmark treatment (1.147, Panel B) than in the strategy information treatment (0.654, Panel B). Consistent with H2, linked measures (2.071) have a statistically significantly ($t = 3.93$, $p = 0.01$) greater mean impact than non-linked measures (0.654), but only when participants are provided detailed information on business unit strategy (Panel C). Finally, as predicted by H3, in the absence of detailed strategy information, common non-linked measures marginally statistically significantly dominate unique linked measures ($t = 1.34$, $p = 0.09$, one-tailed, Panel D). Conversely, when participants are provided with detailed strategy information, unique linked measures marginally statistically significantly dominate common non-linked measures ($t = 1.34$, $p = 0.09$, one-tailed, Panel D).

V. CONCLUDING REMARKS

We conducted an experiment to determine the impact that explicit and detailed strategy information has on the use of strategically linked performance measures in conjunction with common and unique measures. Our results confirm those of Lipe and Salterio (2000) and Slovic and MacPhillamy (1974), as well as our own hypotheses. Taken together, this research suggests that, *ceteris paribus*, evaluators rely more on common measures than unique measures. However, the amount of strategy information available to the decision maker affects the task of performance evaluation, presumably because information affects the

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[13] Results for the regression specified in Exhibit 7 are available from the authors upon request. Only the mean effects of variables relevant to the hypotheses are included in Exhibit 7.

EXHIBIT 7

Estimated Mean Impact of Various Measure Types on Ratings Difference Computed from OLS Statistical Regression Analysis[a]

$$
\begin{aligned}
W_i - F_i = {} & \alpha_0 + \alpha_1\, CL_i + \alpha_2\, CN_i + \alpha_3\, UL_i + \alpha_4\, UN_i + \alpha_5\, Treatment\ S_i + \alpha_{12}\, CL_i \times CN_i \\
& + \ldots + \alpha_{45}\, UN_i \times Treatment\ S_i + \alpha_{123}\, CL_i \times CN_i \times UL_i + \ldots + \alpha_{345}\, UL_i \times UN_i \\
& \times Treatment\ S_i + \alpha_{1234}\, CL_i \times CN_i \times UL_i \times UN_i + \ldots + \alpha_{2345}\, CN_i \times UL_i \times UN_i \\
& \times Treatment\ S_i + \alpha_{12345}\, CL_i \times CN_i \times UL_i \times UN_i \times Treatment\ S_i + \gamma_1\, Male_i \\
& + \gamma_2\, Age_i + \gamma_3\, WorkExp\text{-}A\&A_i + \gamma_4\, WorkExp\text{-}M\&S_i + \gamma_5\, Retail\ Experience_i \\
& + \gamma_6 Masters\text{-}A\&A_i + \gamma_7 Masters\text{-}M\&S_i + \gamma_8 Years\ in\ US < 5_i + \gamma_9\, Work\ Experience_i \\
& + \gamma_{10}\, Number\ of\ Retail\ Visits + \varepsilon_i
\end{aligned}
$$

Calculation of Mean Impact[b]	Estimated Mean Impact: First Term[c]	Estimated Mean Impact: Second Term[c]	(t-statistic) and Significance for Difference in Mean Impacts
Panel A: Common versus Unique Measures			
Impact C_B − *Impact* $U_B > 0$	1.872	0.802	(2.91) 0.01
Impact C_S − *Impact* $U_S > 0$	1.726	0.999	(2.00) 0.02
Impact CL_B − *Impact* $UL_B > 0$	1.055	0.472	(2.26) 0.01
Impact CL_S − *Impact* $UL_S > 0$	1.290	0.781	(1.98) 0.02
Impact CN_B − *Impact* $UN_B > 0$	0.817	0.330	(1.89) 0.03
Impact CN_S − *Impact* $UN_S > 0$	0.436	0.218	(0.84) 0.20
Panel B: Impact of Strategy Information (H1)			
Impact L_B − *Impact* $L_S < 0$	1.527	2.071	(1.48) 0.07
Impact N_B − *Impact* $N_S > 0$	1.147	0.654	(1.36) 0.09
Impact C_B − *Impact* $C_S = 0$	1.872	1.726	(0.40) 0.69
Impact C_B − *Impact* $U_S = 0$	0.802	0.999	(0.55) 0.58
Panel C: Linked versus Non-Linked Measures (H2)			
Impact L_B − *Impact* $N_B = 0$	1.527	1.147	(1.04) 0.30
Impact L_S − *Impact* $N_S > 0$	2.071	0.654	(3.93) 0.01
Impact CL_B − *Impact* $CN_B = 0$	1.055	0.817	(0.93) 0.35
Impact CL_S − *Impact* $CN_S > 0$	1.290	0.436	(3.34) 0.01
Impact UL_B − *Impact* $UN_B = 0$	0.472	0.330	(0.56) 0.58
Impact UL_S − *Impact* $UN_S > 0$	0.781	0.218	(2.19) 0.02
Panel D: Unique Linked versus Common Non-Linked Measures (H3)			
Impact UL_B − *Impact* $CN_B < 0$	0.472	0.817	(1.34) 0.09
Impact UL_S − *Impact* $CN_S > 0$	0.781	0.436	(1.34) 0.09

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[a] Variable definitions: $W - F$ is the difference in a participant's rating of The Women's Store and The Family Store. *CL*, *CN*, *UL*, and *UN* are indicator variables that take the value of 1 if The Women's Store outperforms The Family Store on the specified measure type in the scorecard version given to participant *i*, and 0 otherwise. *Treatment S*, is an indicator variable that takes the value of 1 if the participant was assigned to the strategy information treatment and 0 if the participant was assigned to the benchmark treatment. *Male* equals 1 if the participant is male, and 0 otherwise. *Age* is the participant's age in years. *WorkExp-A&A*, *WorkExp-M&S*, and *Retail Experience* take the value of 1 if a participant has had full-time work experience related to accounting, auditing or taxation, marketing or sales, and the retail industry, respectively; and equal 0 otherwise. The variables *Masters-A&A* and *Masters-M&S* take the value of 1 if a participant's likely area of emphasis will be in accounting, auditing, or taxation, or if it will be in marketing or sales, respectively; and equal 0 otherwise. *Years in US* < 5 equals 1 if the participant has lived in the U.S. for less than 5 years. *Work Experience* is the participant's full-time work experience in years and *Number of Retail Visits* is the number of times the participant had visited a retail clothing store in the past 12 months.

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(*continued on next page*)

EXHIBIT 7 (continued)

[2] *Impact* $C_{TREATMENT}$ is the estimated mean impact of common measures in the *TREATMENT* indicated. It is calculated by subtracting the mean of the average ratings differences ($W_i - F_i$) for scorecard versions in which The Women's Store is not favored on all common measures from the mean of the average rating differences for scorecard versions in which The Women's Store is favored on all common measures. The variables *Impact* $U_{TREATMENT}$, *Impact* $L_{TREATMENT}$, *Impact* $N_{TREATMENT}$, *Impact* $CL_{TREATMENT}$, *Impact* $CN_{TREATMENT}$, *Impact* $UL_{TREATMENT}$, and *Impact* $UN_{TREATMENT}$ are the estimated mean impacts of unique, linked, non-linked, common linked, common non-linked, unique linked, and unique non-linked measures, respectively. There variables are calculated analogously to *Impact* $C_{TREATMENT}$.

[3] "First Term" refers to the value of the first term in the expression in each row of the first column ("Calculation of Mean Impact") and "Second Term" refers to the value of the second term. For example, in the first row, the first term reports the estimated mean impact of common measures under the benchmark treatment, and the second term indicates the estimated mean impact of unique measures under the benchmark treatment. The mean impacts of measure types reported in this exhibit are not identical to those reported in Exhibit 6 because this exhibit includes the influence of control variables in its estimation. Without the demographic control variables, the mean impacts of various measure types calculated with regression are identical to those calculated in the means tests.

[4] One-tailed t-tests when inequality is specified, two-tailed t-tests when equality is specified.

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decision maker's understanding of strategic linkages. Managers who have detailed strategy information will rely more on strategically linked performance measures and less on non-linked measures than those with less knowledge of SBU strategy. In particular, when managers have detailed strategy information, they will rely more on strategically linked measures, even if they are unique, than on non-linked measures that are common.

Our results are subject to certain limitations. First, our experiment participants did not necessarily have experience in the retail clothing industry or extensive experience with the BSC. However, manipulation checks suggest that participants adequately understood the case materials and the task they were asked to perform. Second, our participants did not face the same incentives as managers normally responsible for evaluating subordinates' performance. Third, the additional information provided to participants in the strategy information treatment was quite explicit, it was presented immediately prior to performing the task, and it did not compete with much information for the participant's attention. Our intent in presenting the case information in this manner was to make the key objectives of strategy clear to the reader in order to create a strong contrast between the treatments. However, this may have led experiment participants to place greater importance on strategic information than managers would have placed on such information in practice. While this does not vitiate our findings, it does suggest that it is essential for those implementing the BSC to ensure that managers understand SBU strategy and how scorecard measures relate to that strategy. Despite these limitations, this study still has important implications for those companies that have, or are considering, implementing the BSC.

Our findings imply that managers must understand the linkages between performance measures and business unit strategy in order to benefit from the adoption of the BSC. While our results lend support to Lipe and Salterio's (2000) finding that "experimental participants succumb to the simplifying strategy of using only common measures when evaluating multiple managers," the results are also reassuring for BSC adopters. The tendency of evaluators to rely on common measures does not necessarily undermine the role of the BSC in reinforcing business strategy because evaluators also rely on strategically linked performance measures, especially when they understand the business unit's strategy. Thus, our findings suggest an important precondition for the successful implementation of a BSC: The performance evaluators must understand the links between different elements of the business strategy in order to identify and rely on strategically linked measures when evaluating performance.

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How Do Analysts Use Their Earnings Forecasts in Generating Stock Recommendations?

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ABSTRACT: This paper examines whether valuation estimates based on analysts' earnings forecasts are consistent with their stock recommendations. Because earnings forecasts are linked to value and recommendations reflect analysts' opinions of value relative to current price, earnings forecasts and stock recommendations should be linked in a predictable manner. I consider four possible valuation models of how earnings forecasts and stock recommendations are linked. These models include two specifications of the residual income model, a price-earnings-to-growth (PEG) model, and analysts' projections of long-term earnings growth. The results provide little evidence that analysts' recommendations are explained by either residual income model specification. However, both the PEG model and analysts' projections of long-term earnings growth explain analysts' stock recommendations. The relation between the valuation models and future returns is also examined. Analysts' projections of long-term earnings growth have the greatest explanatory power for stock recommendations, but investment strategies based on these projections have the least association with future excess returns. Overall, the evidence suggests that analysts' recommendations are more correlated with heuristic valuation models than with present value models, and buy-and-hold investors would earn higher returns relying on present value models that incorporate analysts' earnings forecasts than on analysts' recommendations.

Keywords: *stock recommendations; earnings forecasts; valuation; residual income model; PEG; earnings growth.*

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I. INTRODUCTION

Prior research on sell-side security analyst outputs focuses primarily on earnings forecasts, prompting Schipper (1991) to call for a better understanding of how earnings forecasts are related to stock recommendations. This paper directly examines the link between earnings forecasts and stock recommendations. Several previous studies infer that recommendations serve as indicators of "intrinsic" value relative to market value (e.g., Stickel 1985; Womack 1996), and numerous studies document an association between market value and earnings forecasts (e.g., Abdel-khalik and Ajinkya 1982; Givoly and Lakonishok 1984). I hypothesize that valuation estimates based on analysts' earnings forecasts and financial statement data explain the distribution of analysts' stock recommendations. If so, then buy (sell) recommendations should be observed for stocks deemed undervalued (overvalued) based on valuation estimates relative to current prices.[1] This is the first study to directly examine the link between analysts' earnings estimates and stock recommendations via valuation models.

To examine whether earnings forecasts and recommendations are linked, it is necessary to specify *how* they might be linked. Presumably, analysts use their earnings forecasts along with other information to estimate a stock's value, which is then compared to the actual trading price of the stock and forms the basis for the recommendation. Based on evidence in prior research, I consider four possible models of how earnings forecasts might be linked to stock recommendations. First, recent research demonstrates that residual income valuation approaches are both theoretically and empirically meritorious in identifying mispriced securities (Ohlson 1995; Frankel and Lee 1998). For example, Frankel and Lee (1998) implement several versions of the residual income model, incorporating analysts' forecasts of earnings and earnings growth, and demonstrate that such valuations can be used to reliably predict future cross-sectional returns (i.e., identify mispricing). Because the model is sensitive to terminal value specifications (Lundholm and O'Keefe 2001), I consider two versions of the residual income model, which differ in their assumptions regarding earnings growth at the terminal value forecast horizon (V_{RI1} assumes residual income fades; V_{RI2} assumes residual income persists).

As an alternative to the two residual income valuations, I also consider two valuation heuristics identified in recent research. Block (1999) surveys financial analysts and finds extremely low reliance on present value valuation methods (i.e., dividend discount, residual income, etc.). Similarly, Bradshaw (2002) examines a small sample of analyst research reports and finds that analysts rarely provide valuations obtained from the residual income or dividend discount models. Instead, analysts frequently support their recommendations with a price-earnings-to-growth model, PEG. The third valuation model considered is a PEG valuation, computed as forecasted earnings per share times the projected long-term growth rate (V_{PEG}).[2] Finally, I also consider analysts' long-term growth (*LTG*) projections in isolation. Although not a valuation model per se, the growth projection is included as a "valuation model" because survey results in Block (1999) and the content analysis in

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[1] Casual empiricism and prior research indicate that analysts rarely issue sell recommendations. As a result, the market appears to interpret hold recommendations as essentially sell recommendations. The cross-sectional research design accounts for this well-known optimistic bias in recommendations.

[2] The PEG ratio is discussed in detail in Section III.

Bradshaw (2002) both suggest analysts favor growth as a primary determinant of favorable recommendations.[3]

Even though Block (1999) and Bradshaw (2002) find little evidence from questionnaires and content analyses that analysts report the results of present value methods as justification for stock recommendations, this does not necessarily mean they do not use the residual income model or some other valuation model whose results are correlated with those from the residual income model. For example, most of the large brokerage houses actually claim that their analysts *do* use valuation models such as the residual income model.[4] Nevertheless, individual analysts who use present value models may choose to communicate the results of their analyses in the simplest terms, excluding a detailed discussion of present value techniques (i.e., dividend assumptions, discount rates, etc.). Additionally, there are obvious proprietary costs to divulging particular methods of identifying any single security for recommended investment. The use of several alternative valuation models links the current paper to prior research, while also contributing to our limited understanding of the link between earnings forecasts and stock recommendations.

The results provide little evidence that analysts' recommendations are explained by either of the residual income valuations. In contrast, there is strong evidence that both PEG valuations and *LTG* explain analysts' recommendations. Furthermore, the PEG-based valuations retain significant explanatory power for recommendations after controlling for *LTG*, extending the results from the small sample content analysis in Bradshaw (2002) to a larger population. The finding that analysts' recommendations are explained by the PEG heuristic is complemented by recent theoretical and empirical results that demonstrate how the PEG valuation model can be justified in terms of the dividend discount model (Ohlson and Juettner-Nauroth 2000; Easton 2002). The overall tenor of the results is that analysts give the highest recommendations to growth stocks, and among growth stocks, analysts give the highest recommendations to those where the value of growth estimated by the PEG model exceeds current price.

Regardless of how buy/sell signals are generated, their usefulness is ultimately determined by their ability to direct investors toward investments that earn excess market returns. Tests of the association between the four valuation metrics and future excess returns confirm prior findings that residual income valuations are useful in identifying stocks with future excess returns (Frankel and Lee 1998). Both specifications of the residual income model sort stocks according to excess returns over one-year-ahead return measurement periods. The PEG valuation model also sorts stocks according to future excess returns for the sample, but this appears due to incomplete controls for risk. Despite having the greatest explanatory power for analysts' recommendations, *LTG* actually predicts *negative* excess returns for the sample, echoing results in La Porta (1996) and Dechow and Sloan (1997). Finally, analysts' recommendations themselves are uncorrelated with future excess stock returns.

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[3] Further, because of the importance of the long-term growth projection in the PEG valuation, any correlation between analysts' recommendations and PEG-based valuations could be spurious, merely reflecting analysts' use of their long-term growth projections as the basis for their recommendations. Considering growth as an alternative addresses the possible spurious correlation between recommendations and PEG valuations through the growth factor.

[4] For example, in support of economic value added or residual income analysis, Salomon Smith Barney (1998) argues that these methods help to "logically quantify our estimates of the intrinsic value of these companies, and make judgments with regard to the attractiveness of investing in these companies." Additionally, in its "Apples to Apples" reports, Morgan Stanley Dean Witter (1998) argues, "Unlike single-period value measures...the residual income approach considers future periods' profitability. Further, unlike DCF, a relatively small portion of value is normally found in the terminal value. Many concerns about accounting measurement will have a reduced impact on valuation, because of a built-in offset over time."

Overall, the examination of the link between earnings forecasts and stock recommendations via valuation models contributes to our understanding of how sell-side analysts utilize their own forecasts of accounting earnings as an input to their recommendations. The results of the paper can be summarized with the following table:

Correlation between:

	V_{RI1}	V_{RI2}	V_{PEG}	*LTG*	*REC*
Recommendations	–	0	+	++	NA
Future excess returns	+	+	+	–	0

The primary implication is that analysts do not incorporate their earnings forecasts into their stock recommendations in a manner consistent with present value models, but instead rely on valuation heuristics. Implications of the future returns results are less clear. Recommendations are (1) uncorrelated or negatively correlated with residual income valuations that predict positive future excess returns, (2) positively correlated with PEG valuations that also predict positive future excess returns, but (3) positively correlated with *LTG*, which is actually a negative predictor of future excess returns. The strongest explanatory variable for recommendations is *LTG*, which is a strong predictor of future negative returns. Together, these results imply that analysts favor stocks with high-growth expectations even though such expectations have already been impounded into prices. Thus, the results present a conundrum: the evidence suggests that investors might as well ignore analysts' recommendations, but this conclusion stands in contrast to recent studies concluding the opposite, that analysts' recommendations are associated with future excess returns (e.g., Womack 1996; Barber et al. 2001).

The conflicting results can be explained by the different methodologies employed between this study and recent studies. First, this study focuses primarily on recommendation levels and examines buy-and-hold returns over one-year holding periods. Both Womack (1996) and Barber et al. (2001) examine changes, not levels, and Womack (1996) finds significant returns primarily over short-window intervals. Barber et al. (2001) extend Womack (1996) by examining recommendation levels over longer windows. However, their design assumes monthly rebalancing, which, in addition to prohibitively high transaction costs, appears to largely compound series of short-window returns as documented by Womack (1996). Although conflicting with recent studies, the results are actually consistent with a large number of earlier studies on stock recommendations. Perhaps due to differing samples and methodologies, the evidence is mixed overall, but the majority of studies do not find recommendations to have investment value beyond short-term horizons.[5] For example, Logue and Tuttle (1973) conclude, "an investor who routinely follows the advice of his brokerage firm would do, on balance, as well by randomly selecting securities" but "it appears that such advice is not generally harmful."

The paper proceeds as follows. Section II discusses relevant prior research and the hypotheses. The implementation of the valuation models and methodology are discussed in Section III. Section IV describes the data and descriptive statistics. Results are discussed in Section V. Section VI concludes and conjectures on why analysts might report recommendations based on heuristics rather than present value models.

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[5] For examples of earlier papers that conclude stock recommendations do lead to profitable investment strategies, see Groth et al. (1979), Bauman et al. (1995), and Kim et al. (1997). For examples of papers that conclude stock recommendations do not lead to profitable investment strategies, see Cowles (1932), Diefenbach (1972), Logue and Tuttle (1973), Bidwell (1977), Wright (1994), and Desai and Jain (1995).

II. PRIOR RESEARCH AND HYPOTHESIS

Prior studies examine implications of accounting information for stock prices and how intermediaries such as financial analysts facilitate the flow of such information to the capital markets. Analysts' earnings forecasts provide information relevant to expectations of future cash flows, the primary determinants of common stock valuations (Bernard 1994; Dechow et al. 1998). Furthermore, admitting the possibility that prices can deviate from intrinsic values, analysts' stock recommendations reflect opinions on the relative degree of under- or overvaluation (Cowles 1932; Bidwell 1977; Womack 1996). For example, Womack (1996, 164) concludes that stock recommendations can be understood as an analyst stating, "I have analyzed the publicly available information, and the current stock price is not right."

Although there is extensive research on both earnings forecasts and stock recommendations, evidence linking these two analyst outputs is limited. If earnings forecasts are important inputs into useful recommendations, then earnings forecasts and recommendations should be related in a predictable manner. For example, Schipper (1991) argues that earnings forecasts are "not a final product but rather an input into generating a final product." While several papers provide evidence through simple associations between earnings forecasts and value or between value and recommendations, none explicitly examine valuation models that might be the mechanism by which analysts transform earnings forecasts into stock recommendations.

Previts et al. (1994) review 479 analysts' reports with the WordCruncher computer program to document information needs of analysts, and find extensive use of various accounting data in their reports. However, Block (1999) and Bradshaw (2002) find small sample evidence inconsistent with analysts using present value models in picking stocks. Nevertheless, Bandyopadhyay et al. (1995) examine price forecasts for 128 Canadian firms and investigate the explanatory power of earnings forecast revisions for stock price forecast revisions. They find that near-term forecasted earnings revisions explain approximately 30 percent of the variation in price forecast revisions, but long-term earnings forecast revisions explain approximately 60 percent of the variation in price forecast revisions. However, they do not examine stock recommendations. In a related paper, Francis and Soffer (1997) examine the relative price informativeness of earnings forecast revisions and changes in stock recommendations. They find that both signals are informative after controlling for the other. Finally, Stickel (1998) and Finger and Landsman (1998) investigate whether analysts take advantage of previously documented market anomalies or common information metrics in setting their recommendations. They find that analysts' recommendations are significantly correlated with some of these variables (e.g., book-to-market, price momentum, earnings momentum), but the signs of the correlations are inconsistent with analysts taking advantage of the anomalies (e.g., more favorable recommendations for low book-to-market stocks).

If the objective of analysts' recommendations is to advise the purchase (sale) of stocks when the price is low (high) compared to their estimate of intrinsic value, then recommendations should be related to value estimates based on their earnings forecasts.[6] However, analysts' estimates of value are generally unobservable. Thus, a critical exercise in this study is estimating value using the earnings forecasts. In an effort to mimic the analysts' unobservable valuations, I construct value estimates based on two specifications of the residual income model and one specification of the PEG model. Detailed discussion of these models is deferred to the next section.

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[6] As Graham and Dodd (1951, 404–405) state, "The analyst could do a more dependable and professional job of passing judgment on a common stock if he were able to determine some objective value, independent of the market quotation, with which he could compare the current price. He could then advise the investor to buy when price was substantially below value, and to sell when price exceeded value."

Scaling the value estimates (V) by price (P) results in a V/P ratio that provides a distribution of the relative attractiveness of stock investments, which is compared to the distribution of the associated stock recommendations. In addition to the three valuation models, I also consider analysts' growth projections as a separate "valuation model" based on analysts' demonstrated emphasis on growth (e.g., Block 1999). The general hypothesis (in alternative form) is:

> **H_0:** Analysts' recommendations are more (less) favorable for relatively higher (lower) earnings-based estimates of value.

It is important to note that the spirit of the analysis is relative, not absolute. Because the optimistic bias in recommendations is well known (Stickel 1995), I do not expect to observe sell recommendations if $V/P < 1$. The recommendations data reflect the *mean* consensus, which exhibits a more continuous distribution than the *median* consensus or individual discrete recommendations (i.e., strong buy, buy, and hold; also see footnote 18). Thus, the analyses test for a cross-sectional positive correlation between recommendations and valuations rather than applying a strict benchmark such as $V/P = 1$.

In addition, by investigating three valuation models as well as analysts' growth projections (*LTG*) as possible explanatory variables for how analysts' earnings forecasts are linked to stock recommendations, the research design allows competing hypotheses. While there are other models that might be considered, the selected models provide a balance between tractability and coverage of models identified in the literature (i.e., residual income models) and models shown in small-sample studies to be correlated with recommendations (i.e., *PEG* and *LTG*).[7]

III. VALUATION MODELS

Residual Income Specifications

Ohlson (1995) demonstrates that the dividend discount model can be reformulated as follows (firm subscripts omitted):

$$\begin{aligned} P_t^* &= BVPS_t + \sum_{\tau=1}^{\infty} \frac{E_t[EPS_{t+\tau} - r*BVPS_{t+\tau-1}]}{(1+r)^\tau} \\ &= BVPS_t + \sum_{\tau=1}^{\infty} \frac{E_t[RI_{t+\tau}]}{(1+r)^\tau} \end{aligned} \tag{1}$$

where P_t^* is the per share intrinsic value of the stock at time t, $BVPS$ is book value per share, EPS is earnings per share, r is the equity cost of capital, and $EPS_{t+\tau} - r*BVPS_{t+\tau-1}$

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[7] In unreported analyses, I also considered valuations based on industry P/E multipliers. However, due to noisy estimates of the appropriate multiplier (e.g., Baker and Ruback 1999), these valuation estimates had no explanatory power for recommendations and are omitted from the paper.

is residual income (RI).[8] RI reflects earnings in excess of a benchmark level based on an appropriate return on invested capital (i.e., book value). Future book values are extrapolated from historical book values using the clean surplus relation (i.e., $BVPS_t = BVPS_{t-1} + EPS_t - DPS_t$, where DPS = dividends per share). It is assumed that firms continue their recent dividend payout policies, consistent with prior research (e.g., Frankel and Lee 1998).

The model is the basis for the first two valuation estimates because (1) it incorporates expected earnings, and (2) prior research demonstrates its ability to identify mispriced stocks (Frankel and Lee 1998). Equation (1) assumes: (1) the dividend discount model is descriptive of equilibrium prices and (2) clean surplus accounting is followed.[9] The dividend discount model has long been accepted by academics and practitioners.[10] Violations of clean surplus (e.g., accounting for certain foreign currency adjustments, fair values of securities available for sale, and certain pension adjustments) are generally inconsequential. Lundholm (1995) notes that the clean surplus relation "does not precisely match the present state of [GAAP], but it is a very reasonable approximation."

The empirical adaptation of the residual income model is restricted to a finite forecast horizon, imposing additional assumptions beyond the two listed above. Equation (1) is empirically estimated as the present value of expected residual income for the next five years plus a terminal value, calculated as of the end of the fifth forecast year (TV_{t+5}),

$$V_t = BVPS_t + \sum_{\tau=1}^{5} \frac{E_t[RI_{t+\tau}]}{(1+r)^{\tau}} + \frac{E_t[TV_{t+5}]}{(1+r)^5} \tag{2}$$

where V_t is the estimate of intrinsic value, P_t^*. A five-year horizon corresponds to the horizon of analysts' long-term earnings growth forecasts, which are used to extrapolate earnings forecasts. The terminal value estimate presents the greatest empirical challenge because (1) long-term forecasts are generally imprecise, (2) TV can represent a substantial portion of the value estimate, and (3) TV is sensitive to assumptions regarding the behavior of residual income after the terminal value year. To address different procedures that might be used by analysts, I consider two different assumptions regarding the evolution of residual income subsequent to the terminal value year. The first assumption is that residual income fades toward zero (denoted as V_{RI1}); the second assumes that residual income persists as a perpetuity (V_{RI2}).

Residual Income Valuation with a Fade-Rate Assumption (V_{RI1})

The fade-rate assumption is appealing based on economic theory (e.g., Hirshleifer 1980) and empirical evidence (e.g., Penman 1991). Both strands of literature suggest that in the long-run, competitive pressures drive abnormal profits, or residual income, to zero. V_{RI1} is conservative relative to V_{RI2} due to the economic assumption that competition drives residual income to zero. If accounting is not unbiased, then positive residual income may persist for longer periods, favoring V_{RI2}, discussed below.

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[8] Analysts' forecasts provide expectations. Ohlson (1995) demonstrates how additional assumptions regarding "other information" (i.e., linear information model) and the time-series behavior of residual income allow price to be estimated in terms of current observables (e.g., current residual income, book value, etc.). Prior attempts to incorporate this other information into empirical adaptations have met with limited success (Dechow et al. 1999; Myers 1999).

[9] Technically, a regularity assumption is also assumed and requires that in the long run, book value grows at a rate less than r $\left(\text{i.e., } \lim_{\tau\to\infty} \frac{BVPS_{t+\tau}}{(1+r)^{\tau}} = 0\right)$.

[10] Thus, even if analysts are not aware of the residual income model, they are likely to be aware of the dividend discount model. Properly implemented, either model should provide the same valuation (Lundholm and O'Keefe 2001).

To quantify the rate of reversion of residual income, I estimate the following regression by industry using all observations with book value, earnings before extraordinary items, and market value (all for at least two consecutive years) on Compustat during the ten years preceding the sample period:

$$RI_t = \eta + \omega RI_{t-1} + \varepsilon_r \tag{3}$$

Realized residual income is computed as income before extraordinary items (Compustat data item #18) cleansed of special items (item #17) assuming a 35 percent tax rate, less a capital charge based on industry costs of capital (described below) times beginning book value (item #60), all divided by beginning market value (item #25 * item #199). Income is adjusted for special items to be consistent with forecasted earnings, since analysts typically do not forecast special or extraordinary items (Bradshaw and Sloan 2002). The untabulated mean estimate of the autoregressive parameter ω is 0.68, and industry-specific estimates range between 0.51 and 0.80.[11] An ω of 0.68 implies that return on equity reverts to the cost of capital (i.e., RI reverts to zero) over approximately ten years (i.e., $0.68^{10} = 0.02 \approx 0$), which is consistent with empirical evidence in Bernard (1994). Assuming that residual income after the terminal value year is characterized by the industry-specific estimates of ω yields the following terminal value:

$$E_t[TV_{t+5}] = \frac{\omega}{1 + r - \omega} E_t[RI_{t+5}]. \tag{4}$$

This formula is a derived based on the fact that an infinite geometric series of the form $ak + ak^2 + ak^3 + \cdots = ka/(1 - k)$, if $k < 1$, which is true here. In Equation (2), the terminal value is composed of the evolution of $a = E_t[RI_{t+5}]$ and $k = \omega/(1 + r)$. Substituting Equation (4) into Equation (2) yields:

$$V_{RI1,t} = BVPS_t + \sum_{\tau=1}^{5} \frac{E_t[RI_{t+\tau}]}{(1 + r)^\tau} + \frac{\omega E_t[RI_{t+5}]}{(1 + r - \omega)(1 + r)^5}. \tag{5}$$

Discount rates are based estimates in Fama and French (1997, Appendix A), who compute 48 industry-specific risk premia using five-year rolling regressions of excess returns on the excess market return and two mimicking portfolios that proxy for size and book-to-market premia.[12] The industry discount rates, r, are calculated as the industry market premium plus the risk-free rate, proxied by the 30-day Treasury bill yield in effect for the month prior to the recommendation date.

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[11] The mean autoregressive parameter estimate of 0.68 is slightly higher than 0.62 calculated by Dechow et al. (1999), and is primarily attributable to my removal of special items from earnings. Eliminating the effect of "temporary" special items increases the persistence of the earnings measure. When special items are not removed from earnings, the mean autoregressive parameter estimate falls to 0.63. Additionally, the intercept is −0.01 (compared to −0.02 in Dechow et al. [1999]). Although statistically significant, its value is small relative to the coefficient on residual income and is not considered in the terminal value estimates.

[12] These risk premia predate the sample period in this study. As discussed later, alternative discount rates have little impact on the results.

Residual Income Valuation with Perpetuity Assumption (V_{RI2})

The second specification of the residual income model assumes that residual income in the terminal value year persists in perpetuity:

$$E_t[TV_{t+5}] = \frac{E_t[RI_{t+5}]}{r}. \tag{6}$$

This assumption is *inconsistent* with prior research documenting mean reversion in ROE (Bernard 1994). Moreover, it is mathematically equivalent to a finite-horizon dividend discount model with a terminal value equal to a simple earnings capitalization model for period $t+5$ earnings (i.e., EPS_{t+5}/r). Nevertheless, in contrast to the fade-rate assumption embedded in the calculation of V_{RI1}, a perpetuity assumption allows any earnings growth over the forecast horizon to have a large impact on the value estimate. In addition, if accounting is biased, a fade-rate assumption yields conservative value estimates (Feltham and Ohlson 1995). Consequently, the resulting value estimate is more optimistic in its assumptions regarding future earnings than the fade-rate model, but permits a contrasting benchmark for investigating how analysts' earnings forecasts are linked to stock recommendations. Substituting Equation (6) into Equation (2), the second residual income valuation estimate is computed as follows:

$$V_{RI2,t} = BVPS_t + \sum_{\tau=1}^{5} \frac{E_t[RI_{t+\tau}]}{(1+r)^{\tau}} + \frac{E_t[RI_{t+5}]}{r(1+r)^5}. \tag{7}$$

The Price-Earnings-to-Growth Model (V_{PEG})

Block (1999, 92) surveys financial analysts and concludes, "The most important conclusion from this survey is that PV techniques are not as widely used in practice as they are in theory." Block's (1999) survey results are also supported by a content analysis of analysts' reports in Bradshaw (2002), which documents prevalent use of a valuation heuristic seen frequently in the investment community, the PEG ratio, defined as:

$$PEG = \frac{P/E}{LTG}, \tag{8}$$

where P/E is the forward price-to-earnings ratio (i.e., price divided by forecasted earnings), and LTG is the analyst's projection of long-term annual earnings growth. Many investors use this heuristic to assess whether a stock's forward P/E ratio is "out of line." The rule-of-thumb is that PEGs around 1 are Holds, below 0.5 are Strong Buys, and above 1.5 are Strong Sells (e.g., Gerstein 2002). Setting Equation (8) equal to 1 under the assumption that the price is at equilibrium and rearranging the terms to solve for price, one obtains the heuristic valuation model:

$$P_t^* = E_t[EPS]*LTG*100.^{13} \tag{9}$$

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[13] As an example, John G. Kinnard (1999) states, "[we] believe the company deserves a PEG ratio near the top of the industry given its market dominance, broad product line and customer base, growth initiatives, and solid financial position. We value Dialogic's shares based on one times its long-term growth rate of 15 percent. We are initiating coverage of DLGC with a BUY rating for aggressive growth investors and a $32 price target based on 15 times our 2000 EPS estimate of $2.14" (report dated January 15, 1999). The actual trading price as of the date of the report was $24.50, well below the analyst's $32 valuation, which justifies the BUY recommendation.

The third valuation estimate is calculated as a PEG valuation using the two-year ahead earnings forecast:

$$V_{PEG,t} = E_t[EPS_{t+2}]*LTG*100. \tag{10}$$

The results are qualitatively similar if V_{PEG} is calculated using a one-year-ahead earnings forecast, but not as strong as those reported. This is consistent with analyst emphasis on longer term earnings estimates as documented by Bandyopadhyay et al. (1995).

IV. DATA

The analyst data are from the First Call New Real Time Earnings Estimates History Database. This database contains consensus earnings forecasts, projections of long-term earnings growth, and stock recommendations.[14] Monthly observations with consensus stock recommendations and earnings per share forecasts for at least the next two years are merged with Compustat data (book value and dividend payout) and CRSP data (shares and market price).[15] The final sample comprises 12 subsamples partitioned by the one-year-ahead earnings forecast horizon to control for serial correlation in forecasts as the end of the forecast period approaches (e.g., Matsumoto 1999; Brown 2001; Richardson et al. 2001). The main sample consists of 46,209 firm-month observations over the period January 1994–June 1998, representing 4,421 distinct firms.

To make interpretation of the results more intuitive, I invert the standard coding of recommendations to be 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. This permits the more natural interpretation that a higher recommendation suggests possible undervaluation and a buy signal, as does a higher V/P ratio. Thus, the predicted relation between earnings-based valuations (i.e., V/P) and recommendations is positive.

All variables are stated on a per share basis. According to First Call, forecast data are tracked on the same basis used by the company when it reports earnings, which for most companies is *diluted* earnings per share. If analysts take possible share dilution into consideration when analyzing stocks, then per share valuation estimates that ignore dilution could be overstated. Unfortunately, the First Call database does not include a code for the reporting basis of the analysts' earnings forecasts. To gauge whether dilution might pose a problem in this study, I matched sample firms with dilution factors from the I/B/E/S database. The results of this analysis indicate that dilution factors are equal to 1.000 for 86 percent of the observations. Based on this analysis, it seems unlikely that results may be significantly impacted by possible dilution factors.

To implement the residual income model, I use earnings forecasts for up to five years and require that earnings forecasts be available at both one- and two-year horizons. If longer horizon forecasts are unavailable, then they are extrapolated by applying LTG to the most recent forecast available (e.g., $E[EPS_{t+3}]$ = Analyst forecast of EPS_{t+2} times $(1 + LTG)$). If LTG is unavailable, then earnings forecasts are imputed such that return on equity (ROE) for the latest forecast year remains constant over the forecast horizon (e.g., $E[EPS_{t+3}]$ = Analyst forecast of EPS_{t+2} divided by $BVPS_{t+1}$, which is forecasted ROE_{t+2}, times $BVPS_{t+2}$). With less than 100 percent dividend payout, this assumption implies growth in earnings. Future book values are extrapolated under the clean surplus assumption and assume the firm maintains its historically observed payout ratio, proxied by the payout ratio of the most recent fiscal year or the mean payout ratio over the previous three years if the prior year ratio is unreasonable (e.g., less than 0 or greater than 1). Approximately 79 percent of the sample have earnings forecasts for only two years, 20 percent have forecasts for three years, 1 percent have forecasts for four years, and less than 1 percent have forecasts for all five years; approximately 33 percent of the observations have *LTG* projections.

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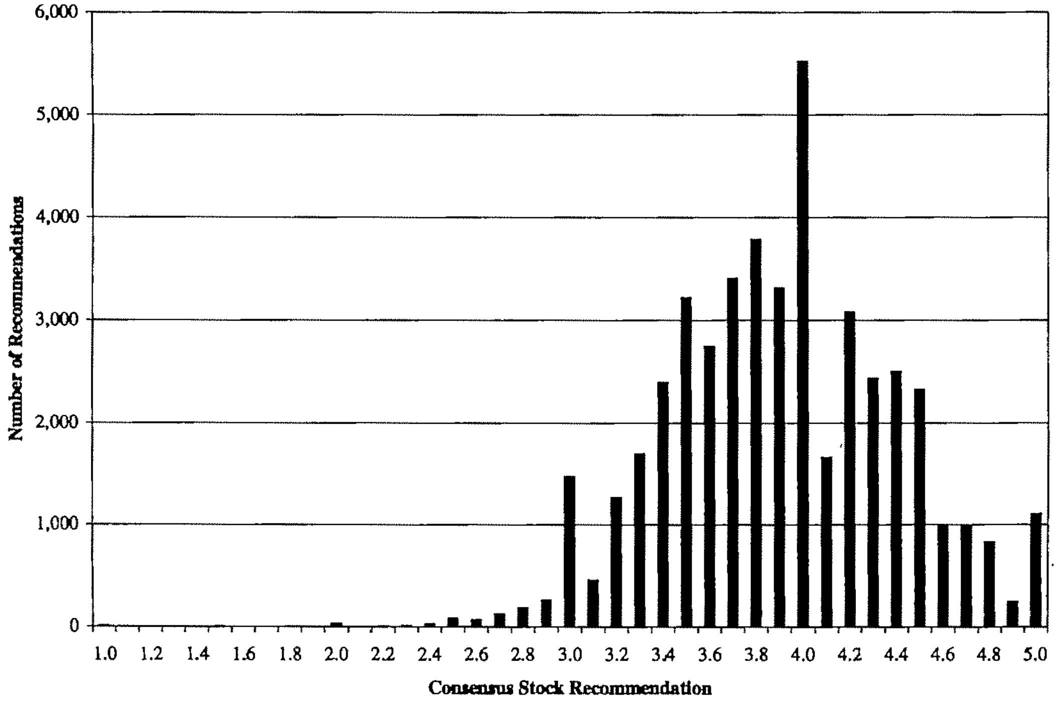
[14] First Call recalculates consensus data whenever there has been a change in the consensus. If a firm-month contains no revisions, the First Call database will not include a consensus for that firm-month. Thus, the consensus forecasts and recommendations used in this study all reflect a change in the consensus.

[15] Because the First Call database is updated continuously as individual analysts update forecasts and recommendations, there are frequently multiple consensus estimates within a given month. I retain the last consensus estimate within each month. All results are insensitive to obtaining the first consensus estimate in each month.

Figure 1 presents the distribution of consensus recommendations. The optimism documented in prior research for sell-side analysts is evident for this sample as well (Stickel 1995). The central tendency of the distribution is close to 4 (i.e., buy), rather than 3 (i.e., hold), which would be expected if the distribution were symmetric. A negligible 1.7 percent of the recommendations are sell recommendations. Because there are relatively few sell recommendations, rational investors essentially interpret hold recommendations as sell recommendations (Morgan and Stocken 2003).

Table 1 presents distributions of the number of observations across fiscal years (Panel A) and descriptive statistics (Panel B). The data show an increasing sample size across each year with the exception of 1998, due to the data extending only through June of that year. In Panel B, the mean and median consensus recommendations (*REC*) are both 3.87.

FIGURE 1
Distribution of Consensus Recommendations



The figure presents the distribution of the pooled mean consensus recommendation. The data span the years 1994–1998. Sample size is 46,209 observations. Recommendations are coded as follows: 1 = Strong Sell, 2 = Sell, 3 = Hold/Neutral, 4 = Buy, and 5 = Strong Buy.

TABLE 1
Descriptive Statistics for Sample Observations for 1994–1998

Panel A: Distribution of Sample across Years

	Frequency		Cumulative	
Year	n	%	n	%
1994	7,908	17.1	7,908	17.1
1995	8,849	19.1	16,757	36.2
1996	10,558	22.9	27,315	59.1
1997	12,805	27.7	40,120	86.8
1998	6,089	13.2	46,209	100.0

Panel B: Pooled Descriptive Statistics for the Full Sample

Variable	n	Mean	σ	Minimum	Median	Maximum
REC	46,209	3.87	0.49	1.00	3.87	5.00
N_{REC}	46,209	13.86	9.60	1.00	11.00	56.00
V_{RI1}/P	46,209	0.67	0.48	0.00	0.59	4.99
V_{RI2}/P	46,209	0.96	0.73	0.00	0.77	5.00
V_{PEG}/P	15,318	1.30	0.79	0.00	1.13	4.99
LTG	15,318	0.17	0.10	0.00	0.15	3.75
r	46,209	0.11	0.04	0.04	0.11	0.18
MV	46,209	3,763	10,125	6	874	239,539
B/M	46,209	0.46	0.28	0.00	0.41	3.74
DP	46,209	0.20	0.23	0.00	0.14	0.90

Panel A presents the distribution of observations across years, where Year is the calendar year in which forecasts and recommendations were released/outstanding. Panel B presents descriptive statistics for selected variables.
Variable Definitions:

REC = consensus (mean) analyst recommendation, ranging between 1 to 5, with 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. If multiple consensus recommendations are calculated by First Call within a calendar month, then only the last one is retained;

N_{REC} = number of analysts' recommendations included in the consensus recommendation;

P = share price on the date of the consensus recommendation calculation;

V_{RI1} = residual income valuation with a five-year forecast horizon and a terminal value with a fade-rate assumption. See Equation (5) in the text;

V_{RI2} = residual income valuation with a five-year forecast horizon and a terminal value with a perpetuity assumption. See Equation (7) in the text;

V_{PEG} = forecasted earnings per share at a two-year forecast horizon times *LTG*. See Equation (10) in the text;

LTG = consensus (median) projected long-term growth in earnings;

r = industry cost of capital estimates based on Fama and French (1997) three-factor estimates of the industry-specific equity risk premia and the risk-free rate in effect for the month prior to the release of the forecasts and recommendations. The risk-free rate is proxied by the 30-day Treasury Bond yield;

MV = market value in $ millions as of the end of the prior fiscal year (data item #25 * data item #199);

B/M = book-to-market ratio, calculated as common equity book value (data item #60) as of the end of the of the prior fiscal year divided by *MV*; and

DP = dividend payout ratio as of the prior fiscal year, calculated as total common dividends (data item #21) divided by income before extraordinary items (data item #237).

The mean (median) number of recommendations (N_{REC}) included in each consensus recommendation is 13.86 (11.00). There is wide variation in the number of recommendations used to compute the consensus recommendation, from a low of 1 to a high of 56. Roughly

1.9 percent (873) of the observations include a consensus recommendation derived from a single recommendation. Nevertheless, none of the results presented are sensitive to excluding consensus recommendations based on just one recommendation.

Mean V_{RI1}/P is 0.67 and mean V_{RI2}/P is 0.96, in line with prior research covering the sample period (Lee et al. 1999).[16] However, the difference in the two residual income valuations demonstrates the importance of terminal value in the calculations and emphasizes the benefit of considering both valuations in the tests. Mean (median) V_{RI2}/P, which assumes residual income in the terminal value year persists forever, is 43 (31) per cent larger than V_{RI1}/P, which assumes residual income fades toward zero subsequent to the terminal value year. Given that the mean recommendation is a buy (i.e., $3.87 \approx 4.00$ = buy) and that both V_{RI1}/P and V_{RI2}/P are less than 1, the descriptive statistics suggest that it is unlikely that analysts' recommendations are based strictly on such valuations, although this does not preclude the predicted positive cross-sectional relation. On the other hand, mean V_{PEG}/P is 1.30, significantly greater than 1 (p-value = 0.001). In contrast to the residual income valuations, PEG valuations exceed current prices, on average, and appear to justify the mean buy recommendations noted above.

Mean *LTG* is 0.17, but ranges from 0.00 to 3.75. Note that *LTG* is available for only one-third of the total observations (i.e., n = 15,318 versus 46,209). Descriptive statistics are very similar if the entire sample is restricted to just the subset of observations that have *LTG* forecasts available. For example, restricting the sample to the 15,318 observations with *LTG* forecasts yields the following means: $REC = 3.87$, $N_{REC} = 17.6$, $V_{RI1}/P = 0.63$, and $V_{RI2}/P = 0.98$. All of these are similar to the full sample means, with the exception of N_{REC}, which is significantly higher for the subset with *LTG* forecasts, consistent with the existence of *LTG* forecasts being correlated with greater analyst following.

Mean and median discount rates are 11 percent, with a range from 4 percent to 18 percent. These discount rates compare to a range of 8 percent to 16 percent in Frankel and Lee (1998), who use a constant risk-free rate. It is evident that the sample firms primarily consist of the largest firms. The mean market value (*MV*) of the sample firms is \$3.76 billion, relative to a Compustat mean (for years 1994–1998, not reported) of \$1.0 billion. The sample firms reflect lower book-to-market (*B/M*) than the population of Compustat firms (0.46 versus 0.68). Mean dividend payout (*DP*) is 0.20 compared to 0.24 for the Compustat population.

V. RESULTS

Univariate Portfolio and Correlation Results

Descriptive evidence for recommendation portfolios is presented in Table 2. Each month during the sample period, observations are independently allocated in equal numbers to quintiles based on the distribution of consensus stock recommendations in that month. For each portfolio, Table 2 provides pooled means and medians for recommendations and each of the valuations. The portfolio formation yields a range in mean consensus recommendations from 3.20 to 4.56, roughly equivalent to a hold to buy/strong buy range. For the least favorable recommendations (portfolio 1), mean V_{RI1}/P is 0.70, higher than the mean V_{RI1}/P of 0.60 for the most favorable recommendations (portfolio 5). For the second residual income valuation, V_{RI2}/P, there is an inverted U-shape to the means across portfolios, with the highest mean occurring in portfolio 3 (mean = 1.03).

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[16] Observations with V/P less than 0 or greater than 5 are deleted. Excluding the 244 extreme observations that fall outside this range does not have a material effect on any inferences.

TABLE 2
Means of Valuations and Long-Term Growth Projections for Recommendation Portfolios

Recommendation Portfolio		*REC*	V_{RI1}/P	V_{RI2}/P	V_{PEG}/P	*LTG*
1	mean	3.20	0.70	0.87	0.92	0.12
(less favorable)	median	3.25	0.66	0.75	0.78	0.10
2	mean	3.60	0.68	0.97	1.14	0.14
	median	3.60	0.62	0.77	1.00	0.12
3	mean	3.86	0.68	1.03	1.32	0.16
	median	3.86	0.60	0.81	1.15	0.15
4	mean	4.13	0.66	1.01	1.49	0.19
	median	4.12	0.56	0.78	1.29	0.17
5	mean	4.56	0.60	0.95	1.60	0.23
(more favorable)	median	4.50	0.52	0.76	1.43	0.22

In each calendar month, all observations are assigned to quintiles based on the distribution of the consensus recommendation in that month. Quintiles are then pooled across time and means for selected variables are presented. Sample sizes across portfolios vary depending on the variable being measured; for *REC*, V_{RI1}/P, and V_{RI2}/P, the sample size per quintile ranges between 9,217 and 9,256, and for V_{PEG}/P and *LTG*, the sample size per quintile ranges between 2,690 and 3,330.

Variable Definitions:

REC = consensus (mean) analyst recommendation, ranging between 1 to 5, with 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. If multiple consensus recommendations are calculated by First Call within a calendar month, then only the last one is retained;

P = share price on the date of the consensus recommendation calculation;

V_{RI1} = residual income valuation with a five-year forecast horizon and a terminal value with a fade-rate assumption. See Equation (5) in the text;

V_{RI2} = residual income valuation with a five-year forecast horizon and a terminal value with a perpetuity assumption. See Equation (7) in the text;

V_{PEG} = forecasted earnings per share at a two-year forecast horizon times *LTG*. See Equation (10) in the text; and

LTG = consensus (median) projected long-term growth in earnings.

In contrast to the residual income valuations, which show no apparent positive relation to recommendation portfolios, there are monotonic patterns for both V_{PEG}/P and *LTG* across portfolios. For example, the least recommended stocks show a mean V_{PEG}/P of 0.92 relative to a mean value of 1.60 for the most favorable recommendation. Moreover, means for V_{PEG}/P tend to be greater than 1 for all but the least favorable recommendations. Similarly, mean *LTG* is 0.12 for the least favorable recommendations, and 0.23 for the most favorable recommendations. Overall, the portfolio means are consistent with recommendations being correlated with PEG valuations and *LTG*, but not with residual income valuations.

Table 3 presents a correlation matrix for the primary variables of interest: *REC*, V_{RI1}/P, V_{RI2}/P, V_{PEG}/P, and *LTG*. The correlations between *REC* and these variables are monotonically increasing in this order. All correlations are significant, with the exception of the correlation between *REC* and V_{RI2}/P (p-value = 0.9964). The correlation between *REC* and V_{RI1}/P is ***negative*** and significant, consistent with analysts issuing the most (least) favorable recommendations for stocks with the lowest (highest) valuations relative to current price. The remaining correlations confirm that V_{PEG}/P and *LTG*, but not V_{RI1}/P or V_{RI2}/P, are positively correlated with recommendations. The strongest correlation is between *REC* and *LTG*, consistent with analysts favoring stocks with high expected growth.[17]

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[17] It is important to keep in mind that (1) analysts are also the forecasters of *LTG* and (2) prior research suggests that realized growth is systematically less than forecasted growth (Dechow and Sloan 1997).

TABLE 3
Correlation Matrix for Recommendations and Valuations

	REC	V_{RI1}/P	V_{RI2}/P	V_{PEG}/P	LTG
REC	—	−0.12	0.00[a]	0.27	0.42
V_{RI1}/P	−0.19	—	0.74	0.48	−0.22
V_{RI2}/P	−0.01	0.79	—	0.50	−0.07
V_{PEG}/P	0.32	0.37	0.47	—	0.37
LTG	0.48	−0.36	−0.12	0.50	—

This table presents Pearson (Spearman) correlations above (below) the diagonal.

[a] All correlations are significant at the 0.01 level, with the exception of the correlation between *REC* and V_{RI2}/P, which is insignificant. Sample sizes are 46,209 for all correlations not including V_{PEG}/P or *LTG*; sample size between V_{PEG}/P or *LTG* and other variables is 15,318.

Variable Definitions:

REC = consensus (mean) analyst recommendation, ranging between 1 to 5, with 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. If multiple consensus recommendations are calculated by First Call within a calendar month, then only the last one is retained;

P = share price on the date of the consensus recommendation calculation;

V_{RI1} = residual income valuation with a five-year forecast horizon and a terminal value with a fade-rate assumption. See Equation (5) in the text;

V_{RI2} = residual income valuation with a five-year forecast horizon and a terminal value with a perpetuity assumption. See Equation (7) in the text;

V_{PEG} = forecasted earnings per share at a two-year forecast horizon times *LTG*. See Equation (10) in the text; and

LTG = consensus (median) projected long-term growth in earnings.

Regressions of Recommendations on Valuation Estimates

The univariate relations are tested formally by computing Vuong tests for relative explanatory power among valuation metrics. The following univariate regression is estimated for each valuation metric (*VALUATION*):

$$REC = \alpha_0 + \sum_{t=1995}^{1998} \alpha_{t-1994} Y_t + \alpha_5 VALUATION + \varepsilon, \tag{11}$$

where Y is an annual indicator variable. The annual indicator variables are included to control for any changes in overall levels of recommendations across the sample period. The regressions are estimated using both actual values and quintile rankings for the independent variables. The quintile ranking transformations are scaled to range from 0 to 1, so the coefficients can be interpreted as the change in *REC* expected when moving from the lowest to highest quintile of the independent variable. Quintile rankings also mitigate effects of extreme values of the valuation estimates. Because inferences are the same regardless of the format of independent variables, results are tabulated only for quintile rankings.

The regressions are estimated separately for each of 12 fiscal months to control for systematic differences in earnings as analysts walk down their forecasts during the fiscal year (e.g., Matsumoto 1999), and the mean of the monthly coefficients is presented. t-statistics are based on the standard error of the monthly coefficient estimates, adjusted for estimated serial correlation across the months. The adjustment factor assumes an AR(1) process for serial correlation in the coefficients estimates (see discussion in table footnotes and Abarbanell and Bernard [2000] for details).[18] In a similar manner, Vuong tests are conducted in each of the 12 fiscal months as described in Dechow (1994), and the reported Vuong statistics reflect significance levels of the mean coefficients, adjusted for serial correlation. Finally, the number of monthly regressions for which the coefficient on each valuation variable is significantly greater than zero is tabulated.

Results appear in Table 4. Panel A presents regression results; Panel B presents Vuong tests for relative explanatory power. Sample size for all regressions is 15,318, reflecting the requirement that *LTG* be available. Results for the residual income valuations are similar for the full sample. In Panel A, model 1 indicates a significant negative coefficient on V_{RI1}/P, implying that analysts give the most favorable recommendations to stocks with the *lowest* residual income value relative to price (i.e., stocks the model indicates are overvalued). The coefficient estimate indicates that when one moves from the lowest quintile valuations to the highest quintile, the consensus recommendations actually *decrease* by 0.226, or approximately half of the standard deviation of the consensus recommendations. Model 2 indicates the coefficient on V_{RI2}/P, the residual income specification with perpetuity growth, is insignificant.

The far right column in Panel A of Table 4 presents the total number of months (out of 12) for which coefficients are significantly greater than zero. In no months is the coefficient on V_{RI1}/P significantly positive, and in only one month is the coefficient on V_{RI2}/P significantly positive. Thus, the negative coefficient on V_{RI1}/P is opposite the prediction that recommendations are positively associated with residual income valuations, and the insignificant coefficient on V_{RI2}/P indicates a failure to reject the null.

In contrast to the coefficients on V_{RI1}/P and V_{RI2}/P, the coefficients on V_{PEG}/P and *LTG* are both positive and significant overall and in each monthly regression. For example, the mean coefficient on V_{PEG}/P is 0.424 (t-statistic = 14.2) and the coefficient on *LTG* is 0.560 (t-statistic = 18.3). The magnitude of these coefficients is economically significant relative to the 0.49 standard deviation of consensus stock recommendations. The Vuong statistics confirm that V_{PEG}/P dominates the residual income valuations, and LTG dominates all valuations at explaining variation in consensus recommendations.

Because *LTG* dominates all other variables in Table 4, the significant explanatory power of V_{PEG}/P in the univariate regression may merely capture the fact that V_{PEG}/P is a linear function of *LTG*. In Table 5, I provide the results of a bivariate regression of recommendations on both V_{PEG}/P and *LTG*. In all 12 monthly regressions, coefficients on V_{PEG}/P and *LTG* are significantly positive. However, monthly F-tests confirm that the incremental explanatory power of *LTG* is significantly greater than that of V_{PEG}/P (the mean of these F-statistics is presented). The magnitudes of the coefficients are lower than in the univariate regressions, possibly due to collinearity between the regressors. Nevertheless, V_{PEG}/P is incrementally significant in explaining variation in recommendations, mitigating concerns that the correlation between stock recommendations and V_{PEG}/P is driven by *LTG*.

The above results are based on a levels analysis, which is subject to standard correlated omitted variables problems. To address this issue, I examine the analysis in Table 4 but

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[18] In addition to ordinary least squares, I estimate ordered response logit regressions as a check on possible misspecification using a linear model. The logit regressions are estimated using the same independent variables, but substitute *median* consensus recommendations for the dependent variables, which take on discrete values. Results for the ordered logit specifications provide the same inferences as the ordinary least squares results and are not tabulated.

TABLE 4
Regressions of Recommendations on Three Valuation Estimates and Long-Term Growth and Vuong Tests for Relative Explanatory Power
(n = 15,318)

Panel A: Regressions of Recommendations on Valuation Estimates and Long-Term Growth Forecasts

Model	Intercept	V_{RI1}/P	V_{RI2}/P	V_{PEG}/P	LTG	Adj. R^2	# > 0
1	3.861	−0.226				0.067	0
	221.1***	−15.0‡					
2	3.741		−0.003			0.037	1
	233.3***		−0.2				
3	3.515			0.424		0.152	12
	108.3***			14.2***			
4	3.456				0.560	0.227	12
	106.4***				18.3***		

Panel B: Vuong Tests (a positive Vuong-statistic indicates the variable in the column heading has more explanatory power than the variable in the intersecting row)

	V_{RI1}/P	V_{RI2}/P	V_{PEG}/P	LTG
V_{RI1}/P	—	−7.1***	5.6***	8.9***
V_{RI2}/P		—	7.9***	9.5***
V_{PEG}/P			—	9.3***

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*, **, *** reflect significance at the 0.10, 0.05, and 0.01, respectively. Significance of regression t-statistics are based on one-tailed tests; significance of regression intercepts and Vuong tests are based on two-tailed tests.

‡ Indicates the coefficient is greater than 2 standard deviations from zero, opposite the predicted direction.

Panel A presents the results of regressions of consensus stock recommendations on valuation estimates. Recommendations reflect consensus (mean) analyst recommendation, ranging between 1 to 5, with 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. The regressions are estimated using quintile rankings of the independent variables. The quintile rankings are designated by allocating observations in equal numbers to quintiles within each calendar month based on the distribution of the variable in that month. The quintile rankings are scaled to range between 0 and 1 (e.g., (QUINTILE-1)/4). Regressions are estimated for each month in a firm's fiscal year for which First Call provides consensus earnings forecasts for two years and a consensus recommendation. The table presents mean coefficients for the 12 monthly regressions, and the t-statistics are based on the standard error of the coefficient estimates across the 12 months, adjusted for autocorrelation in the monthly coefficients based on an assumed AR(1) autocorrelation structure. Standard errors are multiplied by an adjustment factor, $\sqrt{\frac{(1+\phi)}{(1-\phi)} - \frac{2\phi(1-\phi^n)}{n(1-\phi)^2}}$, where n is the number of months and ϕ is the first-order autocorrelation of the monthly coefficient estimates. Adjusted R^2s presented are means across the 12 months. The sample size includes observations with sufficient data to calculate all variables, which averages 1,277 and ranges from 1,009 to 2,084 per month. t-statistics greater than 1.812 (2.764) in absolute value are significant at the 0.05 (0.01) level, respectively, under a one-tailed test with 10 degrees of freedom.

The far right column in Panel A tabulates the number of monthly regressions (out of 12) in which the coefficient is significantly positive.

Panel B presents Vuong test statistics for relative explanatory power among the four valuation models. Monthly Vuong coefficients are estimated as described in Dechow (1994). The test statistics in Panel B are based on the significance of mean Vuong coefficients, adjusted for autocorrelation as described above.

Variable Definitions:

V_{RI1} = residual income valuation with a five-year forecast horizon and a terminal value with a fade-rate assumption. See Equation (5) in the text;
V_{RI2} = residual income valuation with a five-year forecast horizon and a terminal value with a perpetuity assumption. See Equation (7) in the text;
V_{PEG} = forecasted earnings per share at a two-year forecast horizon times LTG. See Equation (10) in the text;
P = share price on the date of the consensus recommendation calculation; and
LTG = consensus (median) projected long-term growth in earnings.

TABLE 5
Bivariate Regression of Stock Recommendations on V_{PEG}/P and *LTG*
(n = 15,318)

Intercept	V_{PEG}/P	*LTG*	Adj. R^2	# $V_{PEG}/P > 0$	# *LTG* > 0
3.236	0.049	0.116	0.249	12	12
77.8***	10.9***	20.6***			

H_0: $V_{PEG}/P = LTG$ F-statistic = 18.9 (mean)
p-value < 0.001

This table presents the results of regressions of consensus stock recommendations on V_{PEG}/P and *LTG*. Recommendations reflect consensus (mean) analyst recommendation, ranging between 1 to 5, with 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. See Table 4 notes for descriptions of estimation procedures and variable definitions. The far right columns tabulate the number of monthly regressions (out of 12) in which the coefficients on each variable are significantly positive. The F-statistic reflects the mean of 12 monthly F-statistics for the null hypothesis that the coefficients on V_{PEG}/P and *LTG* are equal. All p-values are < 0.001.

Variable Definitions:

REC = consensus (mean) analyst recommendation, ranging between 1 to 5, with 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. If multiple consensus recommendations are calculated by First Call within a calendar month, then only the last one is retained;

P = share price on the date of the consensus recommendation calculation;

V_{PEG} = forecasted earnings per share at a two-year forecast horizon times *LTG*. See Equation (10) in the text; and

LTG = consensus (median) projected long-term growth in earnings.

substitute changes in both dependent and independent variables. For this analysis, an observation must have lagged consensus forecasts, LTG projections, and stock recommendations issued within the prior 12 months. This requirement reduces the sample size to 6,258. The results of this analysis are presented in Table 6.

As in the levels regression, changes in V_{RI1}/P (V_{RI2}/P) are negatively (not) correlated with changes in recommendations, and there are no coefficients from the monthly regressions in which either coefficient is significant and positive. The change in V_{PEG}/P is not significant overall, but is significant and positive in three of the monthly regressions. In contrast, changes in *LTG* are significantly *positively* correlated with changes in recommendations (significant and positive in ten months). Thus, the changes results provide stronger additional evidence that *LTG* dominates V_{PEG}/P in explaining stock recommendations.

Both the levels and the changes results are ***inconsistent*** with residual income model valuations explaining variation in analysts' stock recommendations. However, the results might be induced by the assumptions embedded in the empirical implementation of the residual income model, or it might be that analysts' recommendations for a subset of the sample *are* consistent with valuations from a residual income model. I examine the robustness of the results by examining the sensitivity of the valuation estimates to various changes in assumptions or whether results differ across various partitions of the sample. For brevity, these results are not tabulated, but discussed briefly.[19] The results are robust to alternative discount rates (e.g., 8 percent, 12 percent, and 16 percent fixed rates, CAPM-based estimates, Fama and French (1992) firm-specific three-factor based estimates), forecast horizons (e.g., 2 years only, 10 years with level earnings, 10 years with growing earnings), and various partitions of the sample (e.g., research-and-development-intensive firms, firms that report persistent earnings series, firms with high correlations between long-window changes in earnings and returns).

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[19] Results are available in Bradshaw (2000).

TABLE 6
Regressions of Changes in Recommendations on Changes in Three Valuation Estimates and Long-Term Growth and Vuong Tests for Relative Explanatory Power
($n = 6{,}258$)

Panel A: Regressions of Changes in Recommendations on Changes in Valuation Estimates and Long-Term Growth Forecasts

Model	Intercept	$\Delta V_{RI1}/P$	$\Delta V_{RI2}/P$	$\Delta V_{PEG}/P$	ΔLTG	Adj. R^2	# > 0
1	−0.003	−0.033				0.011	0
	−0.3	−2.4‡					
2	0.003		−0.038			0.011	0
	0.3		0.0				
3	−0.029			0.002		0.009	3
	−1.3			0.1			
4	−0.097				0.105	0.030	10
	−3.5***				3.9***		

Panel B: Vuong Tests (positive Vuong-statistic indicates the variable in the column heading has more explanatory power than the variable in the intersecting row)

	$\Delta V_{RI1}/P$	$\Delta V_{RI2}/P$	$\Delta V_{PEG}/P$	ΔLTG
$\Delta V_{RI1}/P$	—	0.4	−1.3	3.2***
$\Delta V_{RI2}/P$		—	−1.2	2.1**
$\Delta V_{PEG}/P$			—	3.7***

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*, **, *** reflect significance at the 0.10, 0.05, and 0.01, respectively. Significance of regression t-statistics are based on one-tailed tests; significance of regression intercepts and Vuong tests are based on two-tailed tests.

‡ Indicates the coefficient is greater than 2 standard deivations from zero, opposite the predicted direction.

Panel A presents the results of regressions of changes in consensus stock recommendations on changes in valuation estimates. Changes are calculated when the date of the lagged variable is within 12 months of the contemporaneous variable. The regressions are estimated using quintile rankings of the independent variables. The quintile rankings are designated by allocating observations in equal numbers to quintiles within each calendar month based on the distribution of the variable in that month. The quintile rankings are scaled to range between 0 and 1 (e.g., (QUINTILE-1)/4). Regressions are estimated for each month in a firm's fiscal year for which First Call provides consensus earnings forecasts for two years and a consensus recommendation. The table presents mean coefficients for the 12 monthly regressions, and the t-statistics are based on the standard error of the coefficient estimates across the 12 months, adjusted for autocorrelation in the monthly coefficients based on an assumed AR(1) autocorrelation structure. Standard errors are multiplied by an adjustment factor, $\sqrt{\frac{(1+\phi)}{(1-\phi)} - \frac{2\phi(1-\phi^n)}{n(1-\phi)^2}}$, where n is the number of months and ϕ is the first-order autocorrelation of the monthly coefficient estimates. Adjusted R^2s presented are means across the 12 months. The sample size includes observations with sufficient data to calculate all variables, which averages 1,277 and ranges from 1,009 to 2,084 per month. t-statistics greater than 1.812 (2.764) in absolute value are significant at the 0.05 (0.01) level, respectively, under a one-tailed test with 10 degrees of freedom.

The far right column in Panel A tabulates the number of monthly regressions (out of 12) in which the coefficient is significantly positive.

Panel B presents Vuong test statistics for relative explanatory power among the four valuation models. Monthly Vuong coefficients are estimated as described in Dechow (1994). The test statistics in Panel B are based on the significance of mean Vuong coefficients, adjusted for autocorrelation as described above.

Variable Definitions:

ΔREC = consensus (mean) analyst recommendation, ranging between 1 to 5, with 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. If multiple consensus recommendations are calculated by First Call within a calendar month, then only the last one is retained, and changes reflect changes between the last consensus estimates in different months;

P = share price on the date of the most recent consensus recommendation calculation;

ΔV_{RI1} = change in residual income valuation with a five-year forecast horizon and a terminal value with a fade-rate assumption. See Equation (5) in the text;

ΔV_{RI2} = change in residual income valuation with a five-year forecast horizon and a terminal value with a perpetuity assumption. See Equation (7) in the text;

ΔV_{PEG} = change in PEG-based valuation, computed as forecasted earnings per share at a two-year forecast horizon times *LTG*. See Equation (10) in the text; and

ΔLTG = change in the consensus (median) projected long-term growth in earnings.

Returns Analyses

In this section, I investigate the profitability of recommendations and each of the valuation metrics over one-year windows. It is possible that although recommendations are not correlated with residual income valuations, the correlation between recommendations and PEG valuations and/or *LTG* leads to profitable recommendations through the ability of these metrics to predict future stock returns.

Future buy-and-hold size-adjusted returns (*SAR*) are calculated as:

$$SAR_i = \left[\prod_{t=1}^{252}(1 + r_{it}) - \prod_{t=1}^{252}(1 + r_{size,t})\right] - 1, \qquad (12)$$

where r_{it} is the daily raw return for stock i and $r_{size,t}$ is the daily return of the size decile to which firm i belongs as of the beginning of the fiscal year. Returns are cumulated beginning on the 15th of the month subsequent to the date of the consensus recommendation. A one-year time horizon is chosen based on indirect evidence in Barber et al. (2001) that recommendations are revised or reiterated between one and three times per year.

Table 7 provides the coefficient estimates from the following regression:

$$SAR = \beta_0 + \sum_{t=1995}^{1998} \beta_{t-1994} Y_t + \beta_5 VALUATION + \varepsilon, \qquad (13)$$

where Y_t are annual indicator variables and *VALUATION* is either V_{RI1}/P, V_{RI2}/P, V_{PEG}/P, or *LTG*. The regressions are estimated in a similar manner to those in Tables 4–6. Regressions are estimated by calendar month with annual dummy variables, and the mean coefficients across the 12 months are presented. t-statistics reflect significance of the mean coefficients, adjusted for serial correlation. Panel A presents the results of regressions of one-year ahead returns on the valuations; Panel B provides Vuong tests of relative explanatory power between the variables. The last column in Panel A tabulates the number of monthly regressions in which coefficients are significantly greater than zero. In addition to the valuations, the consensus stock recommendation is also introduced as a benchmark explanatory variable for future returns.

Panel A of Table 7 reveals that consensus recommendations are generally not associated with one-year-ahead returns. The coefficient on *REC* is 0.037 (t-statistic = 1.5). This result echoes other studies that correlate *levels* of recommendations with future returns (e.g., Cowles 1932; Bidwell 1977; etc.). Most studies documenting a correlation between recommendations and future returns use individual recommendation *revisions* (e.g., Bjerring et al. 1983; Stickel 1995; Womack 1996; etc.), and the return cumulation periods are short (e.g., 1–3 months).

Additionally, Panel A of Table 7 replicates prior research demonstrating the predictive ability of residual income model valuations (e.g., Frankel and Lee 1998). Coefficients on both V_{RI1}/P and V_{RI2}/P are positive and significant (i.e., $V_{RI1}/P = 0.143$, t-statistic = 2.3; $V_{RI2}/P = 0.070$, t-statistic = 4.1). V_{PEG}/P is also positively associated with future excess returns (coefficient = 0.050, t-statistic = 2.3). Thus, despite the heuristic nature of the PEG model, results in Tables 4 and 5 suggesting that analysts' reliance on PEG valuations as a basis for their recommendations appears to be a reasonable approach. However, it is likely that these results are due to incomplete controls for risk, discussed below. Finally, the

TABLE 7

Regressions of One-Year-Ahead Size-Decile Adjusted Returns on Three Valuation Estimates and Long-Term Growth and Vuong Tests for Relative Explanatory Power

(n = 15,024)

Panel A: Regressions of One-Year-Ahead Size-Decile Adjusted Returns on Valuation Estimates and Long-Term Growth Forecasts

Model	Intercept	*REC*	V_{RI1}/P	V_{RI2}/P	V_{PEG}/P	*LTG*	Adj. R^2	# > 0
0	−0.114	0.037					0.009	3
	−1.7	1.5						
1	−0.103		0.143				0.022	11
	−3.7***		2.3**					
2	−0.091			0.070			0.018	11
	−6.9***			4.1***				
3	−0.062				0.050		0.016	9
	−4.0***				2.3**			
4	0.039					−0.129	0.007	1
	0.2					−2.7‡		

Panel B: Vuong Tests (positive test statistic indicates the variable in the column heading has more explanatory power than the variable in the intersecting row)

	REC	V_{RI1}/P	V_{RI2}/P	V_{PEG}/P	*LTG*
REC	—	2.0*	2.0*	1.3	−0.6
V_{RI1}/P		—	−0.4	0.3	−3.3***
V_{RI2}/P			—	1.0	−3.5***
V_{PEG}/P				—	−2.2**

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*, **, *** reflect significance at the 0.10, 0.05, and 0.01, respectively. Significance of regression t-statistics are based on one-tailed tests; significance of regression intercepts and Vuong tests are based on two-tailed tests.

‡ Indicates the coefficient is greater than 2 standard deviations from zero, opposite the predicted direction.

Panel A presents the results of regressions of size-decile adjusted stock returns on consensus recommendations and valuation estimates. Size-decile adjusted stock returns are computed as the one-year buy and hold raw return minus the buy and hold return of the NYSE size decile to which the firm belongs. Return cumulation begins on the 15th of the month subsequent to the date of the consensus recommendation and earnings forecasts. The regressions are estimated using quintile rankings of the independent variables. The quintile rankings are designated by allocating observations in equal numbers to quintiles within each calendar month based on the distribution of the variable in that month. The quintile rankings are scaled to range between 0 and 1 (e.g., (QUINTILE-1)/4). Regressions are estimated for each month with available data. The table presents mean coefficients for the 12 monthly regressions, and the t-statistics are based on the standard error of the coefficient estimates across the 12 months, adjusted for autocorrelation in the monthly coefficients based on an assumed AR(1) autocorrelation structure. Standard errors are multiplied by an adjustment factor, $\sqrt{\frac{(1+\phi)}{(1-\phi)} - \frac{2\phi(1-\phi^n)}{n(1-\phi)^2}}$, where n is the number of months and ϕ is the first-order autocorrelation of the monthly coefficient estimates. Adjusted R^2s presented are means across the 12 months. The sample size includes observations with sufficient data to calculate all variables, which averages 1,252 and ranges from 997 to 2,047 per month. t-statistics greater than 1.812 (2.764) in absolute value are significant at the 0.05 (0.01) level, respectively, under a one-tailed test.

The far right column in Panel A tabulates the number of monthly regressions (out of 12) in which the coefficient is significantly positive.

Panel B presents Vuong test statistics for relative explanatory power among the four valuation models. Monthly Vuong coefficients are estimated as described in Dechow (1994). The test statistics in Panel B are based on the significance of mean Vuong coefficients, adjusted for autocorrelation as described above.

Variable Definitions:

REC = consensus (mean) analyst recommendation, ranging between 1 to 5, with 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. If multiple consensus recommendations are calculated by First Call within a calendar month, then only the last one is retained;

P = share price on the date of the consensus recommendation calculation;

V_{RI1} = residual income valuation with a five-year forecast horizon and a terminal value with a fade-rate assumption. See Equation (5) in the text;

V_{RI2} = residual income valuation with a five-year forecast horizon and a terminal value with a perpetuity assumption. See Equation (7) in the text;

V_{PEG} = forecasted earnings per share at a two-year forecast horizon times *LTG*. See Equation (10) in the text; and

LTG = consensus (median) projected long-term growth in earnings.

variable with the greatest explanatory power for analysts' recommendations, *LTG*, is actually ***negatively*** associated with one-year-ahead excess returns (coefficient = −0.129, t-statistic = −2.7), consistent with La Porta (1996) and Dechow and Sloan (1997).

The tabulations of the number of months that each variable is significantly related to future excess returns give some indication of the systematic nature of the relations with returns. Coefficients on V_{RI1}/P and V_{RI2}/P are significant in 11 of 12 months; the coefficient on V_{PEG}/P is significant in nine of 12 months. Finally, coefficients on *REC* and *LTG* are significantly positive in only three and one months, respectively.

Panel B of Table 7 tabulates the Vuong test statistics. The primary message is that the explanatory power of *LTG* for future returns is dominated by V_{RI1}/P, V_{RI2}/P, and V_{PEG}/P, even though Table 4 indicates that *LTG* dominates those variables in its explanatory power for analysts' recommendations. Panel B also provides weak evidence that the residual income valuations have more explanatory power for future excess returns than recommendations (p-value = 0.10). None of the other Vuong tests are significant, consistent with the residual income valuations and PEG valuations having similar explanatory power for future returns over the sample period.

Whereas risk is explicitly incorporated into the residual income model value estimates (i.e., r in Equations (5) and (7)), there is no such inclusion in the PEG model. To examine whether this affects the returns results in Table 7, Table 8 presents mean equity cost of

TABLE 8
Mean Cost of Capital Estimates across Valuation Portfolios

	Ranking Variable				
Portfolio	V_{RI1}/P	V_{RI2}/P	V_{PEG}/P	*LTG*	*REC*
1	0.108	0.115	0.097	0.106	0.108
2	0.113	0.122	0.108	0.109	0.109
3	0.111	0.111	0.108	0.106	0.108
4	0.106	0.104	0.109	0.108	0.109
5	0.104	0.092	0.111	0.104	0.108
Correlation	−0.59	−0.88	0.84	−0.44	0.32
(p-value, n = 5)	(0.2991)	(0.0511)	(0.0725)	(0.4572)	(0.5981)

This table presents the mean of r, the industry cost of capital estimates, across portfolios formed on the basis of the V_{RI1}/P, V_{RI2}/P, V_{PEG}/P, *LTG*, and *REC*. Pearson correlations between the portfolio rank and the cost of capital are presented at the bottom of the table, with p-values based on n = 5 presented in parentheses below the correlations.

Variable Definitions:

r = industry cost of capital estimates based on Fama and French (1997) three-factor estimates of the industry-specific equity risk premia and the risk-free rate in effect for the month prior to the release of the forecasts and recommendations. The risk-free rate is proxied by the 30-day Treasury Bond yield;

V_{RI1} = residual income valuation with a five-year forecast horizon and a terminal value with a fade-rate assumption. See Equation (5) in the text;

V_{RI2} = residual income valuation with a five-year forecast horizon and a terminal value with a perpetuity assumption. See Equation (7) in the text;

V_{PEG} = forecasted earnings per share at a two-year forecast horizon times *LTG*. See Equation (10) in the text;

P = share price on the date of the consensus recommendation calculation;

LTG = consensus (median) projected long-term growth in earnings; and

REC = consensus (mean) analyst recommendation, ranging between 1 to 5, with 1 = Strong Sell, 2 = Sell, 3 = Hold, 4 = Buy, and 5 = Strong Buy. If multiple consensus recommendations are calculated by First Call within a calendar month, then only the last one is retained.

capital estimates across the valuation portfolios.[20] Consistent with risk being explicitly incorporated into residual income value estimates, there is no correlation between the valuation portfolios and r for the V_{RI1} portfolios. Similarly, there is no significant relation between r and LTG or between r and REC. There is actually a significant negative relation between r and V_{RI2} portfolios, but this correlation would predict *lower* future returns for the high V_{RI2}/P stocks, opposite that observed. Finally, there is a strong significant positive relation between r and the V_{PEG} portfolios, suggesting that the excess returns in Table 7 reflect incomplete controls for risk based on size-adjusted returns.

VI. CONCLUSION

This study investigates how analysts' earnings forecasts are related to their stock recommendations. I predict that analysts' stock recommendations are based on the deviation of estimated values from current trading prices. Valuations based on the residual income model are either unrelated or negatively related to recommendations, depending on the specification. However, valuation estimates based on the PEG model are positively related to recommendations. Further, and to a greater extent, analysts' recommendations are strongly related to their growth forecasts regardless of the degree to which such growth expectations are already impounded in prices. The results are consistent for changes in recommendations.

Residual income valuations are positively associated with future excess returns, consistent with prior research. Additionally, PEG valuations are also positively associated with future excess returns, but additional analysis suggests these returns partially capture risk. Long-term growth, upon which analysts appear to rely most heavily for their recommendations, is actually negatively associated with future excess returns. Thus, analysts' recommendations do not appear to be useful signals for buy-and-hold investors.

The paper demonstrates that analysts do not use present value models alone as the basis for their recommendations. This has not been empirically demonstrated in any prior study of which I am aware. Thus, because investors could implement such models on their own, the value of recommendations must lie in the additional information that analysts incorporate beyond that accounted for in present value models (i.e., quality of management, customer loyalty, etc.). However, the results reveal that analysts' recommendations are less useful for buy-and-hold investors than "recommendations" based on the mechanical present value models. Perhaps analysts use such models, but personal opinions or biases dominate their recommendations.

Several caveats apply to the analysis. First, the sample period is 1994–1998, which covers the inception of one of the greatest bull markets in history. The annual value-weighted market returns for each of these years are 0 percent, 36 percent, 21 percent, 31 percent, and 23 percent, respectively. Thus, the finding that analysts appear to rely on growth per se during this period may not generalize to other periods. Second, the evidence in McNichols and O'Brien (1997) suggests the data constraint that a consensus recommendation be outstanding may bias the analysis toward firms for which analysts are most optimistic. Finally, the analysis does not account for the various incentives faced by analysts, such as compensation, access to management, and underwriting relationships.

Overall, analysts' recommendations are highly associated with their long-term growth projections. However, what is surprising is that analysts recommend high-growth stocks without regard to whether growth expectations have already been incorporated into stock prices. Why would experts such as financial analysts rely on growth regardless of priced growth? Perhaps they are aware of naïve extrapolation of earnings growth impounded in security prices (e.g., Lakonishok et al. 1994), but that it is personally costly to state their true valuations for reasons discussed extensively in prior research, including a desire to "curry favor with management" (Francis and Philbrick 1993), enhance investment banking relationships (Lin and McNichols 1998), or "herd" with other analysts (Graham 1999).

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[20] Because the focus of the paper is on the link between earnings forecasts and stock recommendations, not on the PEG model per se, further analysis of how risk is associated with the PEG model is not pursued here.

The evidence that sophisticated market participants such as analysts appear to use valuation heuristics echoes theoretical work by Fischer and Verrecchia (1999), who argue that noise traders (or "heuristic" traders) who overreact to public information trade aggressively, which tempers demand from informed traders. Analogously, if the market prices securities based on heuristics such as PEG and overextrapolates analysts' growth projections, then analysts may be reluctant to express their more sophisticated beliefs, because of the risk that prices may go even higher. In fact, they may even adopt the heuristic themselves when communicating with the market, exacerbating speculative bubbles (e.g., Liu and Song 2001).[21] The correlation between analysts' recommendations and PEG valuations provides some evidence that analysts do consider the degree to which prices already impound growth expectations. However, the low (high) correlation between changes in stock recommendations and changes in PEG valuations (changes in LTG) suggests that growth in and of itself is the dominant factor.

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[21] With hindsight, many market observers now agree that the period during which the sample covers was characterized by irrationally high valuations. Although ultimately an empirical question, it is possible that analyst emphasis on growth forecasts and valuation heuristics such as the PEG ratio actually contributed to high valuations. For example, Gerstein (2002) argues that "[o]ne reason why the 1.00 PEG ratio is so useful is because so many investors believe in it, and more importantly, act on it. It's one thing to talk about how things ought to be in a perfect world. It's another thing to recognize how they really are when lots of imperfect humans vote with their dollars."

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Self-Selection of Auditors and Audit Pricing in Private Firms

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ABSTRACT: Prior research has examined audit pricing for publicly held firms and provided some evidence of a Big 8 premium in pricing. We investigate audit pricing among private firms, and provide evidence that private firms do not pay such a premium on average. The relatively greater degree of dispersion in auditor choice (between Big 5 and non-Big 5 auditors) in our large sample of privately held audit clients allows us to predict the auditor choice for each firm and to control for potential self-selection. We reject the null hypothesis that clients are randomly allocated across Big 5 and non-Big 5 auditors. Using standard OLS regressions, we document a Big 5 premium; however this premium vanishes once we control for self-selection bias. Moreover, we find that client firms choosing Big 5 auditors generally would have faced higher fees had they chosen non-Big 5 auditors, given their firm-specific characteristics. Our results are consistent with audit markets for private firms being segmented along cost-effective lines. Further, our results suggest that auditees in our setting do not, on average, view Big 5 auditors as superior in terms of the perceived quality of the services provided to a degree significant enough to warrant a fee premium.

I. INTRODUCTION

The industrial organization of the public accounting profession has been a topic of considerable interest to both researchers and regulators since the 1970s. Important dimensions involve the issues of product differentiation and the effects of competition on audit pricing. Starting with Simunic (1980), several researchers have examined whether the audit market is segmented, whether such segmentation affects audit pricing, and along what dimensions audit markets are segmented. While it is generally acknowledged that audit markets are segmented into at least two categories, large and small auditors,

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disagreements remain on the other two issues. In this study we compare audit fees across the two auditor groups in a unique setting with the aim of gleaning some new insights into audit pricing, how audit markets work, and along what dimensions the clientele are segmented into Big 5 and non-Big 5 auditees.

In this paper, we focus on a sample of privately held U.K. audit clients. We believe private firms provide an excellent setting to identify and evaluate new dimensions, such as the cost of providing audit services, along which audit markets could potentially be segmented. This setting is particularly appropriate to examine how the Big 5 auditor advantage (if there is one) is viewed when firms face little or no capital-market pressures to hire a "brand-name" auditor. Unlike samples of U.S.-listed firms, where over 80 percent of the clients hire Big 5 auditors, our sample includes fairly equal numbers of clients choosing Big 5 and non-Big 5 auditors. In a setting where there is no threat of anything approaching monopoly power by the larger auditors, clients will pay a premium to obtain the services of a Big 5 auditor only if they believe the quality offered (benefits received) to be superior to an extent sufficient that justifies such a premium.

Further, private firms are a significant portion of the U.K. economy and of the market for audit services. For example, private firms constituted 99.9 percent of all private non-agricultural entities in 1993 Europe (Mulhern 1995). Over 90 percent of registered U.K. companies are private (Companies House, U.K.), and, in our sample of firms, we find that over 50 percent of the audit fees received by U.K. auditors are attributable to private firms. Several recent studies also rely upon the unique or differing attributes of private firms to improve our understanding of issues in financial accounting literature (e.g., Ball and Shivakumar 2002; Beatty et al. 2002; Beaver et al. 2003). Our study is positioned along similar lines.

We expect private firms to have significantly different demand-and-supply functions for audits as compared to listed firms. There are several reasons why a Big 5 fee premium might not be observed among private firms. For example, Big 5 fee premiums may not be observed for these firms if such a fee premium is primarily related to litigation risks, as the risks of litigation are, in general, low for private firms. Similarly, private firms are more closely held than listed firms, and their shareholders take a more active role in management, on average. This closer relationship may reduce agency conflicts as well as the demand for financial statements as a monitoring mechanism (Fama and Jensen 1983). Hence, private firms may not be willing to pay a higher audit fee to obtain the services of a Big 5 auditor.

On the other hand, there are several equally convincing reasons why Big 5 fee premiums might be observed among private firms. First, accounting information may actually play a more important role in the evaluation of managerial performance at private firms for the very reason that these firms lack market measures of firm value. To the extent that the information generated by the accounting system is used to determine personnel and compensation decisions (Blackwell et al. 1994), the quality of financial statements becomes particularly relevant for privately held firms lacking other (market-based) measures. Many private firms are not entirely run by owner/managers, in which case agency conflicts continue to create the need for monitoring of management (Ang et al. 2000). In addition to the potential usefulness of high-quality reporting in monitoring and compensation decisions, private firms concerned about obtaining financing at the lowest possible cost may believe that Big 5 auditors will facilitate that goal (Beatty 1989).

A private firm may wish to cultivate a relationship with a Big 5 auditor to establish a credible measure of value in view of the possibility of going public in the future or of being targeted for acquisition. Further, privately held clients may believe that a Big 5 auditor can provide superior tax expertise or advisory services. Finally, auditor choice may also be influenced by the quality of nonaudit services provided by the auditor. Thus, private firms may attach value to the superior reputation of Big 5 auditors if they believe that the reputation reflects enhanced quality. Given opposing arguments for (and against) the existence of a Big 5 fee premium among private firms, we empirically investigate this issue.[1]

Apart from examining audit pricing in private firms, this paper also addresses econometric issues in the literature. Despite the large number of studies examining audit fee issues, relatively few econometric advances have been made in this area of research. Notable exceptions include Copley et al. (1995) and Ireland and Lennox (2001). Most studies of audit pricing include a dummy variable for the actual auditor choice in the audit fee regressions. However, as suggested by Copley et al. (1995) and Ireland and Lennox (2001), the actual auditor choice is likely to be endogenous in these regressions. Since client firms are not randomly assigned to audit firms, it is probable that firms self-select into Big 5 and non-Big 5 auditees based on firm characteristics, private information, or other unobservable characteristics. We adopt the classical econometric selection model (Heckman 1978; Lee 1979) to address potential bias in the standard OLS regressions due to self-selection.

In addition, most prior studies restrict the slope coefficients in audit fee regressions to be the same across Big 5 and non-Big 5 clients, allowing only the intercept to vary between the two groups. In contrast, we argue that slope coefficients are likely to differ across auditor groups, and we test the empirical validity of this conjecture. More specifically, we suggest that auditors structure their business in a manner appropriate for their specific client segments, with Big 5 auditors investing more in technology, training, and facilities and, as a result, carrying out audits more efficiently for large, relatively complex clients. The costs of these investments result in a relatively high fixed component of audit fees, which may be unattractive (and costly) for small and less complex clients. Thus, we predict that the intercept in audit fee regressions is higher for Big 5 clients, but that the slope coefficients on variables associated with increased levels of audit effort or risk are generally smaller.

Using the two-stage Heckman approach to test for self-selection, we reject a null hypothesis that clients are randomly allocated across Big 5 and non-Big 5 auditors. After controlling for self-selection, we find no evidence of a Big 5 auditor fee premium. On average, client firms in our sample choose auditors that minimize their audit fees. Client-firms that choose Big 5 auditors generally would have faced higher fees had they gone to a non-Big 5 auditor, given their firm-specific characteristics. Further, we find that the slope coefficients in audit fee regressions differ significantly across Big 5 and non-Big 5 auditors, suggesting that Big 5 auditors have a different fee structure from non-Big 5 auditors. Consistent with our arguments on audit market segmentation, the intercept for Big 5 auditors is larger than that for non-Big 5 auditors, while the slope coefficients on proxies for size and complexity tend to be smaller.

Our paper makes several contributions to the literature. First, we show that a Big 5 premium is not observed in a setting where firms do not feel compelled by market pressures to hire a brand-name auditor but face a more balanced choice.[2] Second, we explore the possibility that the market for audit services is segmented along cost-efficient lines and present evidence consistent with this argument. Third, we propose and demonstrate an approach for addressing self-selection issues in the audit fee literature, which may be applied in a number of settings relevant to accounting research where choice variables are included in the research design.

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[1] Still another possibility is that Big 5 auditors may charge lower fees as the result of economies of scale (Simunic 1980; Francis and Stokes 1986). However, economies of scale are likely to extend to all large clients, irrespective of whether the firm is privately held or publicly listed.

[2] This, however, does not necessarily imply that capital-market pressures drive the Big 5 fee premium documented in prior studies for listed firms.

The remainder of the paper is organized as follows. Section II presents the model specification, Section III describes the data and results, and Section IV concludes the paper.

II. MODEL SPECIFICATION

Overview

Most studies examining the existence (or non-existence) of a Big 5 auditor fee premium estimate the following OLS regression:

$$F_i = \beta' X_i + \gamma Big\ 5_i + \varepsilon_i, \tag{1}$$

where F_i is the audit fee and $Big\ 5_i$ is a dummy variable that takes the value 1 if the actual auditor is one of the Big 5 auditors, and 0 otherwise. The remaining explanatory variable (X_i) captures other client and auditor characteristics that affect fees. A significant positive coefficient for γ in the above model is generally interpreted as evidence consistent with the existence of a Big 5 auditor fee premium.[3] This approach, although straightforward, relies on at least two implicit assumptions. First, the regression assumes that the incremental costs of choosing a Big 5 auditor are constant across all firms, which justifies the use of a dummy variable to capture pricing differences across auditor types. This assumption can be restrictive, as it does not allow for interactive effects between auditor type and firm characteristics and ignores differences in the demand for Big 5 auditors. Second, the model implicitly assumes that auditors are randomly allocated to client firms, which rationalizes the inclusion of $Big\ 5_i$ as an exogenous variable in the regression. This paper examines the validity of these assumptions for our sample of private client-firms.

Self-Selection in Auditor Choice

Theoretical studies (Titman and Trueman 1986; Datar et al. 1991) present signaling models to explore the self-selection of auditors by clients, and it is widely accepted in principle that clients self-select their auditors. From an econometric perspective, self-selection introduces a bias in the standard OLS regressions. To address this point more formally, consider the following equations:

$$\text{Auditor choice equation:}\quad \begin{aligned} Big\ 5_i^* &= \alpha' Z_i + u_i \\ Big\ 5_i &= 1 \text{ if } Big\ 5_i^* > 0 \\ Big\ 5_i &= 0 \text{ if } Big\ 5_i^* \leq 0 \end{aligned} \tag{2}$$

$$\text{Audit-fee equation:}\quad F_{0i} = \beta_0' X_i + \varepsilon_{0i} \qquad \text{if } Big\ 5_i = 0 \tag{3}$$

$$F_{1i} = \beta_1' X_i + \varepsilon_{1i} \qquad \text{if } Big\ 5_i = 1 \tag{4}$$

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[3] The above model, as well as the other models discussed in this section, is an application of the treatment effects model where the benefit or cost associated with a particular treatment (defined broadly to include any choice or decision) is evaluated. A central problem in this evaluation literature is the estimation of counterfactuals, which is the expected outcome under the alternative treatment state. The econometric issues relating to these models are extensively examined in the program evaluations literature, and our discussions here draw heavily upon this literature (e.g., Heckman et al. 1999; Moffitt 1991; Maddala 1983, Chapter 9).

where X_i and Z_i are vectors of exogenous variables and the error terms, u_i, ε_0, and ε_{1i} are assumed to be normally distributed with mean zero and variance-covariance matrix given by:

$$\text{Covariance}(\varepsilon_{0i}, \varepsilon_{1i}, u_i) = \begin{bmatrix} \sigma_{00} & \sigma_{01} & \sigma_{0u} \\ \sigma_{01} & \sigma_{11} & \sigma_{1u} \\ \sigma_{0u} & \sigma_{1u} & \sigma_{uu} \end{bmatrix}$$

This self-selection model, which is based on Lee (1979), is quite general in that it allows for simultaneity in the auditor-choice and audit-fee equations, as well as for self-selection. The model also allows slope coefficients and the error terms in the fee equations to vary across auditor types.[4,5]

Allowing slope coefficients to differ across auditor groups is important in our setting as our arguments related to market segmentation imply that the intercept in the fee regressions should be higher for Big 5 clientele, while the slope coefficients on complexity, size, and audit risks should generally be lower.

Standard OLS regressions of the self-selection model are potentially misspecified. To show this, we take the conditional expectations of the residuals in Equations (3) and (4):

$$E(\varepsilon_{0i}|Big\ 5_i = 0) = E(\varepsilon_{0i}|u_i \leq -\alpha' Z_i) = -\sigma_{0u}\left[\frac{\phi(-\alpha' Z_i)}{\Phi(-\alpha' Z_i)}\right] = \sigma_{0u}\lambda_{0i},$$

$$E(\varepsilon_{1i}|Big\ 5_i = 1) = E(\varepsilon_{0i}|u_i > -\alpha' Z_i) = \sigma_{1u}\left[\frac{\phi(-\alpha' Z_i)}{1 - \Phi(-\alpha' Z_i)}\right] = \sigma_{1u}\lambda_{1i}.$$

The functions ϕ and Φ are the standard normal probability density function and the cumulative distribution function, respectively; σ_{0u} and σ_{1u} are the covariances of the residuals from the non-Big 5 and Big 5 audit-fee equations, respectively, and the residuals from the auditor-choice equation. In the above equations, if $\sigma_{0u} \neq 0$ or $\sigma_{1u} \neq 0$, then the conditional expectations are nonzero, causing standard OLS regression to be misspecified.[6] Moreover, in this case, OLS regressions yield biased estimates for the primary parameter of interest, namely the average benefit or cost of auditor choice. To see this, note that the average benefit or cost of auditor choice is given by:

$$\text{For non-Big 5 clients: } \overline{F_0} - E(F_1|Big5 = 0) = \overline{F_0} - \beta_1'\overline{X} - \sigma_{1u}\overline{\lambda_0} \quad (5)$$

$$\text{For Big 5 clients: } \overline{F_1} - E(F_0|Big\ 5 = 1) = \overline{F_1} - \beta_1' - \overline{X} - \sigma_{0u}\overline{\lambda_1} \quad (6)$$

where a bar over a variable denotes its cross-sectional average.[7] If σ_{0u} is positive, then the fee differential estimated from OLS regressions will be biased upward for Big 5 clients as

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[4] The standard simultaneous equations model for Big 5 auditor choice and total audit fees as implemented in Copley et al. (1995) can be viewed as a special case of the above self-selection model (Heckman 1978; Lee 1979). Relative to the self-selection model, the standard simultaneous equations model makes the following additional assumptions: (1) the marginal fees for choosing Big 5 auditors (i.e., fee differential across Big 5 and non-Big 5 auditors) are observable and (2) the coefficients and the error terms in the fee equations are restricted to be the same across Big 5 and non-Big 5 auditors.

[5] Ireland and Lennox (2001) estimate a similar self-selection model, but focus only on differences in intercepts and ignore differences in slope coefficients across the Big 5 and non-Big 5 samples.

[6] Testing whether $\sigma_{0u} = 0$ and $\sigma_{1u} = 0$ is, in fact, the test for self-selection (see Maddala 1983, 259).

[7] In Equations (5) and (6), all variables and firm characteristics that determine fees, including λ_j ($j = 0,1$), are obtained from a particular auditor type. However, the parameters (β_j and σ_{ju}, $j = 0$ or 1) are obtained from the alternative auditor type.

$\bar{\lambda}_1 > 0$. Similarly, if σ_{1u} is positive, then the fee differential estimated from OLS regressions will be biased downward for non-Big 5 clients, as $\bar{\lambda}_0 < 0$.

Implementation and Identification

Using the two-stage procedure of Heckman (1979) and Lee (1979), we estimate the self-selection model. In the first stage, consistent estimates for α are obtained from a probit regression of the dummy variable, *Big 5*$_i$, on Z_i. These estimates are used to compute the inverse Mills ratios (IMR), λ_{0i} and λ_{1i}. Then, in the second stage, the audit fee equation is estimated by OLS with the inverse Mills ratio included as an additional explanatory variable. Our self-selection model is given as:

$$\text{Probit Regression:}\quad Big\ 5_i = \alpha_1 + \alpha_2 Size_i + \alpha_3 Aturn_i + \alpha_4 DA_i + \alpha_5 Curr_i + \alpha_6 Quick_i + \alpha_7 ROA_i + \alpha_8 ROA*Loss_i + \alpha_9 Export_i + u_i \tag{10}$$

$$\text{OLS regression:}\quad Lfee_i = \beta_{j1} + \beta_{j2} Size_i + \beta_{j3} Aturn_i + \beta_{j4} Export_i + \beta_{j5} Curr_i + \beta_{j6} DA_i + \beta_{j7} Quick_i + \beta_{j8} ROA_i + \beta_{j9} ROA_i*Loss_i + \beta_{j10} Yrend_i + \beta_{j11} Yrend_i*Size_i + \beta_{j12} London\_dum_i + \beta_{j13} Abs\_excep_i + \beta_{\lambda j}\lambda_{ji} + \varepsilon_{ji} \tag{11}$$

where:

j = 0 for non-Big 5 client firms, 1 for Big 5 client firms;
$Lfee_i$ = logarithm of audit fees;
Big 5$_i$ = 1 if firm i chose a Big 5 auditor in year *t*, 0 otherwise;
$Size_i$ = logarithm of end of year total assets;
$Aturn_i$ = asset turnover, calculated as sales divided by total assets;
$Export_i$ = sales outside United Kingdom divided by total sales;
$Curr_i$ = current assets divided by total assets;
DA_i (debt-asset ratio) = long-term debt divided by total assets;
$Quick_i$ (quick ratio) = current assets minus inventory divided by current liabilities;
ROA_i = earnings before interest and taxes divided by total assets;
$Loss_i$ = 1 if the firm incurred a loss in the previous year, 0 otherwise;
$Yrend_i$ = 1 if the firm's year-end occurs in either December or March;
$London\_dum_i$ = 1 if the auditor is located in London, 0 otherwise; and
Abs_excep = absolute value of exceptional or extraordinary items included in earnings during the year, divided by total assets.

The coefficients, $\beta_{\lambda 0}$ and $\beta_{\lambda 1}$, are the estimates for σ_{0u} and σ_{1u}, which are the covariances of the residuals from the non-Big 5 or Big 5 audit-fee equation and the residuals from the auditor-choice equation.

The motivation for, and predictions of, our explanatory variables follow. Consistent with previous studies, we hypothesize that auditee size, complexity, and risk affect both fees and the choice of the audit firm. We initially discuss the explanatory variables used in the second-stage audit fee regressions, followed by a discussion of the association between the variables and auditor choice (first-stage regression). The predicted signs for the variables are presented in Tables 3 and 4.

We use the logarithm of total assets and asset turnover (sales divided by total assets to attenuate collinearity with total assets) to control for audit effort. To control for audit risk, we include variables for financial structure and profitability of the client firm. Following Francis (1984), quick ratio and leverage are included to measure the short-term and long-term financial structures of the client firm. Since the relationship between *ROA* (our measure of profitability) and fees is likely to be more important for loss-making firms than for profitable firms, as litigation claims largely follow losses, we allow the coefficient on *ROA* to differ across profit- and loss-making firms. The audits of receivables and inventory require specific audit procedures because these items are viewed as relatively high-risk assets; to capture this, we include current assets as a percentage of total assets as an additional explanatory variable in the regressions. We also include the fraction of sales earned from exports.

The self-selection model allows the slope coefficients and the intercept in the audit fee regression to vary across Big 5 and non-Big 5 clients. This is important since our arguments on market segmentation predict differences in cost functions across these client firms. Specifically, if Big 5 auditors invest more in technology, training, etc., then the intercept in the fee regression of Big 5 clients will be larger, reflecting the Big 5 auditors' compensation for their increased investments. However, this larger investment will allow Big 5 auditors to conduct audits more efficiently, particularly for large, risky, and relatively complex clients. In such a case, we expect the slope coefficients on variables associated with audit effort or risk to be smaller for these clients.

The self-selection model can be estimated only if it satisfies the identification condition, which requires the vectors X_i and Z_i to have elements that are not common. However, the probit regression (10) is identified even in the absence of any exogenous variables in Z_i because of its nonlinearity (see Wills and Rosen 1979). To identify the audit fee Equation (11), we include the following variables in the audit fee equation and not in the auditor choice equation: (1) *Yrend*, a dummy variable equal to 1 if year-end is either March or December, and 0 otherwise; (2) *London_dum*, a dummy variable that takes the value 1 if the auditor office is located in London, and 0 otherwise; and (3) *Abs_excep*, the absolute value of exceptional or extraordinary items included in earnings (scaled by total assets).

The year-end dummy variable is included to control for off-peak pricing. However, since larger clients are often continuously audited throughout the year, the incremental workload around fiscal year-ends may be relatively lower for these firms; thus we allow the year-end coefficient to vary with client size. Following Ireland and Lennox (2001), we include a dummy for London audit offices to control for the relatively higher overhead costs that these auditors are likely to face. Finally, we include the absolute value of exceptional or extraordinary items in earnings to control for both incremental audit risks and audit work related to these items.

For our first-stage (auditor choice) regression, we include the same variables with the exceptions noted above. We expect larger companies to be more likely to hire Big 5 audit firms for various reasons, including the perception that larger auditors are better equipped to handle the audit efficiently. Firms with high asset turnover may be either more or less likely to prefer a large auditor, as this variable may capture transaction complexity or, alternatively, may reflect a decreased necessity to audit large dollar volumes of assets per dollar of sales.

Agency costs tend to be higher in highly leveraged clients, and such firms may prefer to hire auditors of "superior" reputation to reduce agency costs. Also, to the extent that Big 5 auditors have generally wider experience and arguably better-trained personnel, they may be able to audit riskier clients more efficiently. We also expect firms with relatively heavy export activity to prefer Big 5 auditors because larger auditors are better equipped to handle the geographical dispersion and complexity of such audits. We include profitability measures to consider the possibility that auditor choice might be influenced by firm profitability or the presence of a recent loss.

III. INSTITUTIONAL DETAILS, DATA, AND RESULTS

Institutional Background and Data

All limited liability companies in the U.K. can be classified as either public companies or private companies. All limited liability firms are required by the U.K. Companies Act to submit a copy of their full financial statements annually to the Companies House. The financial statements must be prepared in accordance with U.K. GAAP, irrespective of whether the firm is public or private. However, small- and medium-sized companies are required to submit only abridged versions of their financial statements to the Companies House.[8] During our sample period, financial statements submitted to the Companies House must be audited unless annual sales are less than £350,000. Tax laws do not discriminate between public and private companies and, hence, are unlikely to give rise to any differences across these firms.

The data source for this study is the "Financial Analysis Made Easy" (FAME) database supplied by *Bureau Van Dijk*. This database provides accounting data on over 100,000 private British companies for fiscal years ending after January 1989. Since data availability is limited in initial years, we restrict our sample period to the five-year period 1994–1998.[9] The database includes only companies that either have annual turnover in excess of £750,000, pretax profits greater than £45,000, *or* shareholders' funds greater than £750,000. Further, we exclude firms with less than £1 million in total assets, since many of the variables, including audit fees, are rounded to the nearest thousand and their inclusion could introduce significant noise in the analyses. Finally, banks, insurance, and other financial institutions are excluded from the FAME database. We eliminate firms that changed organization type during our sample period (i.e., converted from private to listed or listed to private), as well as any firm-year in which the fiscal year was not exactly 12 months. In addition, we screened out observations for which book value of total assets changed by over 30 percent from the prior year, as these were often firm-years in which a major acquisition, restructuring, or divestment occurred.[10] Our final sample consists of a yearly median of 15,255 (mean of 15,484) private firm observations.

Descriptive Statistics

Table 1 reports descriptive information for our sample of firms. We present the mean and standard deviation for each variable used in our regressions, first for all audit clients

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[8] Small-sized companies have (1) sales turnover of not more than £2.8 million, (2) total assets (book value) of not more than £1.4 million, and (3) average number of employees of not more than 50 for the last two years. Medium-sized companies are larger than small companies and have (1) annual sales revenue of not more than £11.2 million, (2) total assets (book value) of not more than £5.6 million, and (3) an average number of employees of not more than 250 for the last two years.

[9] Fiscal years are converted into calendar years using the same approach as Compustat; that is, fiscal years ending before May 31st are treated as belonging to the previous calendar year, while those ending on or after June 1st are classified as belonging to the same calendar year as the year in which the financial statements are prepared.

[10] Our results remain unchanged whether we include or exclude these observations.

TABLE 1
Descriptive Data

		Total	***Big 5* = 1**	***Big 5* = 0**	**t-statistic (median)**
No. Observations	Mean	15,484	7,729	7,755	
Log of Audit Fees	Mean	2.355	2.641	2.069	40.18
	Std. Deviation	0.016	0.034	0.012	
Audit Fees (000s)	Mean	17.765	24.153	11.365	21.68
	Std. Deviation	0.313	0.934	0.256	
Size (= log of Total Assets)	Mean	8.634	9.111	8.156	49.82
	Std. Deviation	0.051	0.093	0.032	
Total Assets (000s)	Mean	24,280	41,819	6,794	11.12
	Std. Deviation	3155	7088	313	
Aturn (= Sales/Assets)	Mean	1.943	1.820	2.066	−10.35
	Std. Deviation	0.034	0.021	0.043	
Export (= Export/Sales)	Mean	0.142	0.176	0.107	17.84
	Std. Deviation	0.007	0.003	0.010	
DA	Mean	0.111	0.120	0.101	4.84
	Std. Deviation	0.004	0.002	0.006	
Curr	Mean	0.667	0.683	0.652	7.52
	Std. Deviation	0.004	0.002	0.007	
Quick	Mean	1.934	2.407	1.457	1.71
	Std. Deviation	0.470	0.695	0.317	
ROA	Mean	0.040	0.040	0.040	−0.11
	Std. Deviation	0.004	0.002	0.005	
Loss	Mean	0.208	0.230	0.185	6.93
	Std. Deviation	0.009	0.007	0.012	
Yrend	Mean	0.615	0.691	0.540	19.59
	Std. Deviation	0.009	0.004	0.011	
London_dum	Mean	0.415	0.394	0.435	−5.42
	Std. Deviation	0.032	0.064	0.008	
Abs_excep	Mean	0.007	0.008	0.006	2.98
	Std. Deviation	0.004	0.004	0.003	

All means are the average of the yearly means for each variable for the years 1994 through 1998, and the standard deviation is the average of the yearly standard deviations over the same period.

Variable definitions:

Big 5 = 1 if the firm chooses a Big 5 auditor, 0 otherwise;
Export = sales outside United Kingdom divided by total sale;
Size = logarithm of end-of-year total assets;
DA (debt-asset ratio) = long-term debt divided by total assets;
Curr = current assets divided by total assets;
Quick (quick ratio) = current assets minus inventory divided by current liabilities;
Lfee = logarithm of the auditor's fee;
Aturn = sales divided by total assets;
ROA = earnings before interest and taxes divided by total assets;
Loss = 1 if the firm incurred a loss in the previous year, 0 otherwise;
Yrend = 1 if the firm's year ended in either December or March, 0 otherwise;
London_dum = 1 if the auditor is located in London, 0 otherwise; and
Abs_excep = absolute value of exceptional or extraordinary items included in earnings during the year, divided by total assets.

and then separately for auditees of Big 5 and non-Big 5 auditors. The audit fee for the average Big 5 client is £24,153 (≈U.S.$38,645), while the audit fee for the average non-Big 5 client is £11,365 (U.S.$18,184). Means and medians were virtually identical for our sample, so we present only the means of the yearly data. We test for differences between Big 5 and non-Big 5 clients using a t-test, and the medians of the yearly t-tests are presented in the last column of Table 1. As shown in this column, all but two of the variables (*ROA* and *Quick*) are significantly different for Big 5 and non-Big 5 auditees.

The mean of total assets for firms in our sample is £24.3 million (U.S.$38.9 million). In comparison, Francis and Simon (1987) report average total assets of U.S.$30 million for their sample of 220 U.S.-listed firms, while Chan et al. (1993) report average total assets of £233 million for their sample of 985 U.K. quoted firms. For Big 5 auditees, the mean is £41.8 million (U.S.$66.9 million), while for non-Big 5 auditees it is £6.8 million (U.S.$10.9 million). For the average Big 5 auditee, 17.6 percent of their sales come from sales to other countries (exports). This compares to about 10.7 percent exports for non-Big 5 clients in our sample. The average leverage, defined as the ratio of long-term debt-to-total-assets (*DA*), is about 12.0 percent for Big 5 auditees and 10.1 percent for the non-Big 5 clientele. The quick ratio and the ratio of current assets to total assets are higher for clients of Big 5 auditors (although not statistically different for the quick ratio). In addition, the mean return on assets (*ROA*) is not statistically different for Big 5 and non-Big 5 auditees in our sample. Among Big 5 clients, average earnings are negative about 23 percent of the time (*Loss*). This figure decreases to about 18.5 percent for non-Big 5 clients. Big 5 clients have December or March year-ends 69 percent of the time, while only 54 percent of non-Big 5 clients have peak year-ends. Big 5 clients have London-based auditors 39.4 percent of the time, and non-Big 5 clients have auditors from London 43.5 percent of the time. Finally, the absolute value of exceptional or extraordinary items reported in earnings averaged 0.8 percent of total assets for Big 5 clients but only 0.6 percent for non-Big 5 clients.

Table 2 presents the correlations among the variables used in estimating our models. As seen in this table, the correlation between audit fees and client size is 70 percent, and the correlation between client size and the choice of a Big 5 auditor is 35 percent. As expected, the correlation between return on assets and the presence of a loss in the prior year is quite high (−71 percent). Other notable correlations include those between export activity and the variables for firm size (21 percent), audit fees (28 percent), and the choice of a Big 5 auditor (15 percent).

Model Estimation

Auditor-Choice Regression Results

Table 3 presents the results of our estimation of the probit regression described in Equation (10). In predicting auditor choice, all of our explanatory variables are statistically significant in each year of our sample, with the exception of the quick ratio, which is significant in only one of the five years at $p < 0.10$. We find that firms characterized as relatively larger and more highly levered, with a larger portion of assets represented by current assets and with greater foreign involvement, are more likely to choose Big 5 auditors.

To assess the accuracy of our stage one classification, we chose a cut-off level of 50 percent; i.e., if the probability of choosing a Big 5 auditor is greater than 50 percent, then we assume the firm would make that choice. Based on this cut-off, our classification is accurate, on average, 68.5 percent of the time. Also note that approximately half the firms in our sample chose Big 5 auditors each year, with a slightly downward trend in more

TABLE 2
Spearman Cross-Correlations

	Big 5	*Fees*	*Size*	*Aturn*	*Export*	*Da*	*Curr*	*Quick*	*ROA*	*Loss*	*Yrend*	*London_ dum*	*Abs_ excep*
Big 5	1.00												
Fees	0.30	1.00											
Size	0.35	0.70	1.00										
Aturn	−0.08	0.06	−0.19	1.00									
Export	0.15	0.28	0.21	−0.07	1.00								
Da	−0.05	0.05	0.06	−0.11	−0.04	1.00							
Curr	0.08	0.01	−0.10	0.38	0.15	−0.37	1.00						
Quick	0.09	0.02	0.02	−0.08	0.13	−0.17	0.33	1.00					
ROA	0.05	−0.01	0.03	0.05	0.04	−0.15	0.06	0.26	1.00				
Loss	0.04	0.02	−0.04	−0.06	0.00	0.08	−0.02	−0.18	−0.71	1.00			
Yrend	0.14	0.07	0.11	−0.01	0.06	−0.02	0.05	0.02	−0.01	0.03	1.00		
London_dum	0.04	0.11	0.09	−0.01	0.06	−0.02	0.04	0.02	0.01	0.00	−0.02	1.00	
Abs_excep	0.00	0.09	0.09	−0.05	0.02	0.04	−0.05	0.00	−0.04	0.05	0.05	0.01	1.00

For each of years 1994 to 1998, the Spearman cross-correlations are estimated across firms in the sample. The mean correlations across years are presented in the table. The variables are defined in Table 1.

TABLE 3
Demand Equation Estimated by Year

		All Years		1994		1995		1996		1997		1998	
	Hyp. Sign	Mean Estimate	Mean Pr > t	Estimate	Pr > t	Estimate	Pr > t	Estimate	Pr > t	Estimate	Pr > t	Estimate	Pr > t
Intercept		−4.336	0.00	−4.069	0.00	−4.014	0.00	−4.245	0.00	−4.659	0.00	−4.695	0.00
Size	+	0.443	0.00	0.421	0.00	0.416	0.00	0.438	0.00	0.470	0.00	0.468	0.00
Aturn	?	−0.049	0.00	−0.034	0.00	−0.054	0.00	−0.061	0.00	−0.055	0.00	−0.044	0.00
DA	+	0.181	0.04	0.060	0.18	0.146	0.01	0.131	0.01	0.283	0.00	0.284	0.00
Curr	+	0.621	0.00	0.643	0.00	0.587	0.00	0.578	0.00	0.668	0.00	0.631	0.00
Quick	+	0.002	0.29	0.001	0.28	0.004	0.20	0.004	0.07	0.000	0.47	0.001	0.45
ROA	?	1.882	0.00	1.549	0.00	1.743	0.00	2.176	0.00	1.913	0.00	2.027	0.00
*ROA*Loss*	?	−3.962	0.00	−3.531	0.00	−2.819	0.00	−4.668	0.00	−4.158	0.00	−4.635	0.00
Export	+	0.355	0.00	0.239	0.00	0.286	0.00	0.250	0.00	0.431	0.00	0.568	0.00
n =		15,484		14,099		15,255		16,502		16,388		15,176	
% Correctly Classified =		68.5		67.2		67.5		68.8		69.6		69.5	
% Using Big 5		50.1		51.8		50.6		50.1		49.4		48.6	

The probit equation estimated is:

$$Big\ 5_{it} = \beta_1 + \beta_2 Size_{it} + \beta_3 Aturn_{it} + \beta_4 DA_{it} + \beta_5 Curr_{it} + \beta_6 Quick_{it} + \beta_7 ROA_{it} + \beta_8 ROA{*}Loss_{it} + \beta_9 Export_{it} + \mu_{it}$$

where:

Big 5 = 1 if firm *i* chose a Big 5 auditor in year *t*, 0 otherwise;
Size = logarithm of end of year total assets;
Aturn = sales/total assets;
DA (debt-asset ratio) = long-term debt divided by total assets;
Curr = current assets divided by total assets;
Quick (quick ratio) = current assets minus inventory divided by current liabilities;
ROA = earnings before interest and taxes divided by total assets;
Loss = 1 if the firm incurred a loss in the previous year, 0 otherwise; and
Export = sales outside United Kingdom divided by total sales.

recent years (ranging from a high of 51.8 percent in 1994 to a low of 48.6 percent in 1998).[11]

Audit Fee Regression Results

Tables 4 and 5 present the estimates from the regression of audit fees on the explanatory variables. For comparison with prior studies, Table 4 presents the results based on single-stage estimation with the actual auditor choice (*Big 5*) variable included, while Table 5 presents the two-stage regression results using the self-selection model.[12] Since the dependent variable in this regression (i.e., *Lfee*) is highly serially correlated, we do not estimate Equation (2) as a panel regression.[13] We instead estimate Equation (2) on an annual basis and present the means and medians from these annual cross-sectional regressions. Further, our results are generally quite consistent across years, so we do not report individual yearly results.

For the single-stage estimation presented in Table 4, there are 15,255 private firms in the median year with data available for the regressions. For the median year, the regression model explains 57 percent of the cross-sectional variation in audit fees. This adjusted R^2 is comparable to the R^2s reported in earlier studies for listed firms.[14] Further, we find that when the actual auditor choice variable is included as a dummy variable in the regression, the coefficient on auditor choice is significantly positive. The mean coefficient of 0.061 on the Big 5 dummy variable suggests that, on average, Big 5 fees are 6.3 percent higher than non-Big 5 fees.[15] The coefficients on the control variables are generally consistent with our expectations.[16] We discuss the control variables in more detail in subsequent tables.

In Table 5, we present our estimation of regression Equation (11), which is estimated separately for Big 5 and non-Big 5 auditees. This specification includes the inverse Mill's ratio (IMR) from stage one (λ_{0i} or λ_{1i}), which controls for potential self-selection bias in the second stage. The significance of the coefficients on the IMR suggests the importance of controlling for self-selection bias. Further, by estimating the regressions separately for Big 5 and non-Big 5 auditors, our approach allows the slope coefficients to vary across the two groups.

We initially consider the significance of our control variables, as presented in Panel A of Table 5. The signs (and significance) of the coefficients are the same in most cases for Big 5 and non-Big 5 auditee samples. The coefficient on leverage (*DA*) is statistically insignificant. However, the coefficient on the quick ratio, a measure of short-term financial

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[11] The proportion of Big 5 auditors increases monotonically across size deciles for our sample, from a low of 28 percent for the smallest decile of firms to a high of 87 percent for the largest decile, on average.

[12] The t-statistics reported throughout the paper are based on heteroscedasticity-consistent standard errors. Further, for the self-selection model, the standard errors are corrected to account for the fact that an explanatory variable is an estimate from another statistical model. The t-values are based on the method proposed by Greene (1981) to estimate consistent standard errors.

[13] For the same reason, we do not rely on the Fama-Macbeth t-statistic (which assumes observations are serially uncorrelated) as a control for potential cross-correlation in the annual regressions.

[14] For example, Simunic (1980) reports adjusted R^2s from an audit fee regression model of 51 percent for large clients and 28 percent for small clients, on average. Francis and Stokes (1986) report adjusted R^2s of 76 percent for their large client sample and 45 percent for their small client sample.

[15] The economic interpretation for the coefficient on Big 5 dummy is based on the method described in Simon and Francis (1988, 263, footnote 7).

[16] Comparing the coefficients in Table 4 with those reported in Chan et al. (1993) for a sample of 985 U.K.-listed companies from 1989, among variables that are comparable, we observe that the intercept is larger, the coefficient on profitability more negative, and the coefficient on the Big 5 dummy more positive for the listed firms in their sample. The coefficient on client size is similar across the two studies. Although the results in these two studies are not directly comparable because of differences in model specification, the above differences in coefficients are consistent with listed firms facing higher audit fees relative to private firms.

TABLE 4
Single-Stage Audit Fee OLS Regression Coefficients (Using Big 5 Dummy Variable)

	Hyp. Sign	Mean Coefficient	Mean t-value	Median Coefficient	Median t-statistic
Intercept		−2.698	−43.21	−2.713	−42.89
Big 5	?	0.061	5.62	0.062	5.53
Size	+	0.541	75.46	0.544	74.79
Aturn	+	0.072	20.02	0.073	20.74
Export	+	0.300	14.27	0.293	13.86
Curr	+	0.257	11.79	0.248	11.42
DA	?	−0.048	−2.22	−0.051	−2.33
Quick	−	−0.002	−8.22	−0.001	−8.64
ROA	−	−0.236	−3.63	−0.205	−3.55
*ROA*Loss*	−	−0.279	−2.84	−0.217	−2.17
Yrend	+	0.119	1.67	0.116	1.61
*Yrend*Size*	−	−0.017	−2.09	−0.015	−1.84
London_dum	+	0.079	7.73	0.071	7.22
Abs_excep	+	0.300	3.03	0.340	3.47
Adj. R^2		0.565		0.568	

Separate yearly regressions are estimated for years 1994 to 1998, and the mean and median values across the regressions are presented.

The regression estimated is:

$$Lfee_{it} = \beta_1 + \beta_2 Big\ 5_{it} + \beta_3 Size_{it} + \beta_4 Aturn_{it} + \beta_5 Export_{it} + \beta_6 Curr_{it} + \beta_7 DA_{it} + \beta_8 Quick_{it} + \beta_9 ROA_{it} + \beta_{10} ROA_{it} * Loss_{it} + \beta_{11} Yrend_{it} + \beta_{12} Yrend_{it} * Size_{it} + \beta_{13} London\_dum_{it} + \beta_{14} Abs\_excep_{it} + \mu_{it}$$

where:

Big 5 = 1 if the firm chooses a Big 5 auditor, 0 otherwise;
Export = sales outside United Kingdom divided by total sales;
Size = logarithm of end of year total assets;
DA (debt-asset ratio) = long-term debt divided by total assets;
Curr = current assets divided by total assets;
Quick (quick ratio) = current assets minus inventory divided by current liabilities;
Lfee = logarithm of the auditor's fee;
Aturn = sales divided by total assets;
ROA = earnings before interest and taxes divided by total assets;
Loss = 1 if the firm incurred a loss in the previous year, 0 otherwise;
Yrend = if the firm's year ended in either December or March, 0 otherwise;
London_dum = 1 if the auditor is located in London, 0 otherwise; and
Abs_excep = absolute value of exceptional or extraordinary items included in earnings during the year, divided by total assets.

risk, is significantly negative (−0.002 and −0.009 for Big 5 and non-Big 5 samples, respectively). The coefficient on return on assets (*ROA*) is significantly negative, consistent with lower fees for more profitable firms, for the Big 5 sample. However, the coefficient

TABLE 5
Two-Stage Audit Fees Regressions (Controlling for Self-Selection)

Panel A: Regressions for Big 5 and Non-Big 5 Firms

		Big 5 Firms				Non-Big 5 Firms				t-stat for Diff. in Coefficients	
		Mean		Median		Mean		Median		Mean	Median
	Hyp. Sign	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	t-stat	t-stat
Intercept		−1.971	−7.32	−2.010	−7.63	−3.827	−10.86	−3.883	−11.10	4.19	3.81
Size	+	0.481	22.71	0.483	22.36	0.710	13.75	0.721	13.59	−3.98	−3.73
Aturn	+	0.098	15.57	0.115	17.75	0.046	6.49	0.045	5.71	5.55	6.41
Export	+	0.309	9.56	0.304	9.16	0.331	6.34	0.327	6.63	−0.37	−0.22
Curr	+	0.215	4.86	0.197	4.96	0.409	5.62	0.440	5.91	−2.35	−1.95
DA	?	−0.049	−1.71	−0.040	−1.42	−0.044	−0.90	−0.044	−0.99	−0.11	0.09
Quick	−	−0.002	−6.73	−0.001	−6.62	−0.009	−6.29	−0.007	−6.57	3.96	4.07
ROA	−	−0.387	−3.64	−0.374	−3.25	0.370	1.43	0.452	1.66	−2.68	−2.36
*ROA*Loss*	−	−0.016	−0.01	0.053	0.30	−1.693	−3.13	−1.874	−3.65	2.88	2.75
Yrend	+	0.009	−0.09	0.022	0.22	0.052	0.44	0.093	0.77	−0.36	−0.14
*Yrend*SIZE*	−	−0.004	−0.31	−0.007	−0.61	−0.009	−0.64	−0.016	−1.07	0.28	0.01
London_dum	+	0.038	2.58	0.040	2.77	0.115	8.37	0.106	7.62	−3.78	−3.60
Abs_excep	+	0.234	2.43	0.352	3.30	0.610	2.35	0.565	3.64	−0.35	−1.22
Lambda (λ)	?	−0.167	−2.25	−0.184	−2.56	0.508	2.73	0.571	2.74	−2.37	−2.65
Adj. R^2		0.57		0.57		0.46		0.45			
Actual Fee − E(Alt.Fee)		0.82	−220.18	−0.94	−155.67	−0.34	−260.12	−0.38	−281.53		

(continued on next page)

TABLE 5 (continued)

Panel B: F-test for Differences in Slope Coefficients between Big 5 and Non-Big 5

	Mean	**Median**
F-Value	12.03	10.31
Pr. > F	0.0001	0.0001

Panel A reports estimates from the following regression:

$$Lfee_i = \beta_1 + \beta_2 Size_i + \beta_3 Aturn_i + \beta_4 Export_i + \beta_5 Curr_i + \beta_6 DA_i + \beta_7 Quick_i + \beta_8 ROA_i + \beta_9 ROA_i{*}Loss_i + \beta_{10} Yrend_i + \beta_{11} Yrend_{it}{*}Size_{it} + \beta_{12} London\_dum_{it} + \beta_{13} Abs\_excep_{it} + \beta_\lambda \lambda_i + \varepsilon_i$$

where λ is the inverse-Mills ratio.

The rest of the variables are defined in Table 4.

The t-statistics are based on the method proposed by Greene (1981) to estimate consistent standard errors when using the Heckman approach. The mean and median values of the coefficients and t-statistics from yearly regressions are presented for the Big 5 and the non-Big 5 firms in the first six columns. In the last two columns, the mean and median t-statistics from testing the null hypothesis that the individual coefficients are the same for Big 5 and non-Big 5-auditors are presented.

Panel B reports mean and median statistics from a joint F-test that all slope coefficients (with the exception of intercept) in the yearly regressions are the same across Big 5 and non-Big 5 auditors.

on *ROA*Loss* is not significant. For non-Big 5 clients, the opposite is true; i.e., the coefficient on return on assets is not significant, while the coefficient on *ROA*Loss* is significantly negative, suggesting that non-Big 5 auditors do not charge higher fees for less profitable clients *except* in the presence of a prior year loss.

The coefficient on the dummy variable for year-end as well as the coefficient on *Yrend*Size* are statistically insignificant for both the Big 5 and the non-Big 5 clients. We find a positive, statistically significant coefficient on our London office variable. Finally the coefficient on the *Abs_excep* variable is significantly positive, consistent with an expectation that fees are higher when extraordinary or exceptional gains or losses are reported in earnings. In general, the results from the above regressions show that the variables suggested in prior studies as important in explaining the audit fees of listed companies are relevant for private firms as well.

The coefficients on λ_0 and on λ_1 are significant for both the Big 5 and non-Big 5 auditee samples, suggesting that OLS regressions that ignore self-selection will yield biased results. We address this bias directly in the next section.

The mean and median statistics from a joint F-test of whether the slope coefficients (excluding intercept) are significantly different between Big 5 and non-Big 5 client groups are reported in Panel B of Table 5. The coefficients are significantly different in every year 1994 through 1998 at less than the 1 percent level. These results reject empirical specifications that require slope coefficients to be the same across Big 5 and non-Big 5 auditors. In comparing the individual slope coefficients between Big 5 and non-Big 5 client groups, we find that the coefficients for nine of the 14 explanatory variables (including the intercept) differ significantly between the groups. We find significant differences for coefficients on our variables for size, asset turnover, current ratio, quick ratio, *ROA*, *ROA*Loss*, and the London office variable. Of these, the only slope coefficient that is significantly larger in absolute value in the Big 5 client regressions is asset turnover. We speculate that non-Big 5 auditors may focus their efforts and expertise on auditing companies with higher asset turnover; thus, the increment charged for this particular variable is less for non-Big 5 than for Big 5 auditors. The generally larger coefficients on non-Big 5 variables (and significantly lower intercept) are consistent with our expectation that non-Big 5 auditors charge larger increments for measures reflecting increased audit complexity (but a smaller fixed component). This notion is further developed in subsequent paragraphs.

Given the evidence of self-selection bias (and differences in slope coefficients), we evaluate the existence of a Big 5 auditor fee premium in our sample by computing the difference between the actual audit fee paid and the fee that the firm *would have paid,* on average, had the alternative choice on auditor-type been made [E(Alternate fee)]. As mentioned previously, we follow Maddala (1983, Chapter 9) and Heckman et al. (1999), both of which provide detailed discussions of self-selection bias and evaluation problems that require estimation of counterfactuals.[17] We compute this difference in each year and report the mean and median in the last row of Panel A of Table 5. We present the comparative data first for firms actually choosing Big 5 auditors, and then for firms choosing non-Big 5 auditors. The differences are negative in every year, suggesting that if Big 5 auditees had chosen non-Big 5 auditors, their audit fees would have been higher. The difference is statistically significant in every year 1994 through 1998 (not reported). Similarly, in the final comparison of Table 5, Panel A, we find that if non-Big 5 auditees had chosen to hire Big 5 auditors, their fees [E(Alternate fee)] would also have been significantly higher on average than the actual fees paid.

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[17] A counterfactual is any conditional statement (if *X*, then *Y*) in which the antecedent (*X*) is known to be false. For example, if I were a Martian, then I would have a ray-gun. A counterfactual can be useful as an argument for testing the implications of theories and hypotheses, and is a term commonly used in program evaluation literature.

These results suggest that firms, on average, self-select an auditor type that minimizes their audit fees. Explanations for a Big 5 auditor fee premium that are based on superior reputation or deeper pockets of Big 5 auditors imply that firms choosing Big 5 auditors pay higher fees than the amount they would have paid had they chosen a non-Big 5 auditor. However, our results indicate just the opposite and do *not* support the view that, on average, Big 5 auditors charge more than that which *would have been charged by* non-Big 5 auditors, given the firms' characteristics.

Our findings are consistent with audit markets being differentiated along dimensions other than reputation or deep-pockets. More specifically, auditors appear to structure their businesses in a manner that appeals to specific client segments. For instance, Big 5 auditors invest more in technology, training, and facilities, enabling them to carry out audits more efficiently for large, relatively complex clients. However, the fixed costs of these investments may not be attractive to small clients in general. These arguments are supported with the larger intercepts and smaller slope coefficients observed for Big 5 client-firms. Overall, the results are consistent with firms self-selecting the most cost-effective auditor given their firm-specific characteristics.

Robustness Tests

As stated previously, our methodology enables us to deal with two distinct econometric concerns: potential self-selection bias and the possibility that the slope coefficients on the various control variables differ across Big 5 and non-Big 5 auditor groups. Our first robustness test is structured to ascertain whether allowing for slope coefficients to differ between the two client groups would suffice to explain the difference between the traditional OLS regression results presented in Table 4 and our two-stage results presented in Table 5.

We re-estimate the OLS regression as shown in Table 4, but now separately for the Big 5 and non-Big 5 auditor groups. The F-test for a null hypothesis that all slope coefficients are equal across the auditor-groups is rejected in every year. Using the coefficients from these regressions,[18] we next calculate a comparison of actual and expected (alternative) fees based on this specification for our sample of Big 5 clients to see if a Big 5 premium exists. Table 6 presents the benefit or cost of choosing a Big 5 auditor, based on this specification, relative to the fees that the firms would have faced if a non-Big 5 auditor were chosen instead. In Panel A of Table 6, we see evidence of a Big 5 auditor premium when all Big 5 auditor clients are included. Therefore, it is not merely the failure to control for differences in slope coefficients between the samples that explains the difference in results between a single-stage OLS estimation and a self-selection adjusted analysis.

Adjusted for self-selection bias, our results suggest that the unobserved, or surprise, element in auditor choice plays an important role in the audit fee regressions. To test this prediction more directly, we next estimate the above OLS regressions for samples of firms where the predicted auditor choice agrees with (differs from) the actual auditor choice. For this analysis, firms are predicted to choose a Big 5 auditor (non-Big 5 auditor) if the estimated probability from probit regression Equation (10) is greater (lower) than 0.5.

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[18] Apart from the coefficient on the IMR, which is not in this regression, the coefficients are qualitatively similar to those in Table 5 and, hence, are not reported.

TABLE 6
Treatment Effects of Big 5 Auditors

	Mean Values		**Median Values**	
	Difference in Fees	**t-statistic**	**Difference in Fees**	**t-statistic**
Panel A: All Firms				
Actual Fee − E(Alt. fee)	0.040	4.45	0.035	4.42
Panel B: Firms Making Predicted Choices				
Actual Fee − E(Alt. fee)	−0.025	−2.05	−0.023	−2.26
Panel C: Firms Making Unexpected Choices				
Actual Fee − E(Alt. fee)	0.053	4.82	0.068	6.05

The table presents the mean and median values and the associated t-statistics, from yearly computation of the treatment effects. Each year, the treatment effect is measured for Big 5 auditees as the difference in average actual fees paid by these firms and the fees that they would have paid had they selected the alternative auditor-type, E(Alt. fee).

E(Alt. fee) is computed by multiplying model parameters, estimated for Non-Big 5 auditors sample, with averages of explanatory variables for Big 5 auditees. The audit-fee model used to estimate the parameters is:

$$
\begin{aligned}
Lfee_i = {} & \beta_1 + \beta_2 Size_i + \beta_3 Aturn_i + \beta_4 Export_i + \beta_5 Curr_i + \beta_6 DA_i + \beta_7 Quick_i + \beta_8 ROA_i \\
& + \beta_9 ROA_i{*}Loss_i + \beta_{10} Yrend_i + \beta_{11} Yrend_{it}{*}Size_{it} + \beta_{12} London\_dum_{it} + \beta_{13} Abs\_excep_{it} + \mu_{it}
\end{aligned}
$$

where the variables are defined in Table 4.

Panel A reports results from using all firms in the sample, while Panels B and C report results for subsamples of firms. Panel B repeats the analyses for the subsample of firms whose choice of auditor-type is consistent with the predicted auditor-type, while Panel C repeats the analyses for firms whose auditor-choice is not consistent with the predicted auditor type. A firm is classified as making a predicted (unexpected) auditor choice if the auditor type predicted by the probit model reported in Table 3 agrees with (differs from) the actual auditor choice. A firm is predicted to choose a Big 5 (non-Big 5) auditor if the estimated probability from the probit model is greater (less) than 0.5.

The rationale behind these regressions is that for firms whose auditor choice is predicted from observable characteristics, the surprise element in auditor choice is likely to be small and relatively unimportant in audit pricing. For this sample, the self-selection bias in the OLS regressions, if any, is expected to be small. Hence, if the Big 5 fee premium reported in Panel A of Table 6 arises from the surprise element in the self-selection model (i.e., λ_0 and λ_1), then we expect these firms not to bear a Big 5 fee premium. In contrast, the self-selection bias is relatively more important for firms where the predicted auditor choice differs from the actual choice, and so we expect to see a Big 5 fee premium for this sample.

Panel B of Table 6 presents the results for firms making predicted choices, while Panel C presents the results for firms making unexpected choices (based on our stage one model and a 50 percent cut-off prediction). As observed in Table 2, over two-thirds of our sample firms fall into the category of firms making expected auditor choices. For these firms, the OLS regressions do not provide any evidence of a Big 5 auditor fee premium. In fact, the average difference between the Big 5 auditor fee and the expected fee if they had chosen a non-Big 5 auditor is −0.025 (t-statistic = −2.05) for these firms. This negative difference is consistent with our earlier findings that client-firms, in general, choose auditors that minimize their audit fees. However, a Big 5 auditor fee premium is observed for firms making unexpected choices (difference between actual and alternative fee of 0.058, on average, with a t-statistic of 5.21). These results are consistent with our earlier findings based on the Heckman approach and indicate that self-selection resulting from unobservable characteristics significantly biases the OLS estimation of Big 5 auditor fee premiums.

Finally, we examine the sensitivity of our results to nonlinearities in the relationship between audit fees and the explanatory variables by repeating the analyses using rank regressions. In addition, we sort our sample firms into size tertials and repeat the tests. These modifications do not affect our conclusions. We find no evidence of a Big 5-auditor fee premium for any of the size tertials.

IV. CONCLUSION

We investigate audit pricing among private firms in the context of both demand-side and supply-side factors affecting the pricing of audit services. The predominance of private companies in the economy makes a study of their auditor choice decision and audit fee determination interesting in its own right. But more importantly, as suggested by prior studies (Ball and Shivakumar 2002; Beatty et al. 2002), important differences exist between private and listed firms, suggesting that findings documented in the audit pricing literature for listed firms may not necessarily extend to private firms. Our results are consistent with the notion that auditees, when not compelled by market pressures to choose a Big 5 auditor, choose the lowest-cost auditor available; further, our results suggest that clients in our setting, on average, do not view Big 5 auditors as superior in terms of the perceived quality of services provided to a degree sufficient to justify a fee premium.

The paper also addresses econometric issues in the literature for audit pricing. The relatively greater degree of dispersion in auditor choice (between Big 5 and non-Big 5 auditors) in our large sample of privately held U.K. audit clients offers us the opportunity to estimate the predicted auditor choice for each firm and then to use information from this first-stage regression in our fee analysis. In addition, most prior studies restrict the slope coefficients in audit fee regressions to be the same across Big 5 and non-Big 5 clients, allowing only the intercept to vary across the two groups. These studies often implicitly assume that biases arising from self-selection of auditors by client firms are relatively unimportant. This paper examines the validity of these assumptions for our sample of private client firms.

Using the two-stage Heckman approach, we find no evidence of a Big 5 auditor fee premium. However, we do find evidence consistent with firms choosing auditors that minimize their audit fees. We suggest that auditors structure their businesses in a manner appropriate for specific client segments. For instance, Big 5 auditors invest more in technology, training, and facilities, enabling them to carry out audits more efficiently for large, relatively complex clients. However, the fixed costs of these investments may not be attractive to small clients in general. Consequently, private firms self-select, on average, the most cost-effective auditor types, given their firm-specific characteristics.

Finally, we note that this paper examines only the direct costs associated with the selection of a particular auditor type. Since we are unable to measure the indirect effects of auditor choices (such as cost of capital, cost of litigation, etc.), we cannot conclude that private firms necessarily benefit in terms of total costs, but only in terms of audit fees. We leave this issue for future research.

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PE Ratios, PEG Ratios, and Estimating the Implied Expected Rate of Return on Equity Capital

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ABSTRACT: I describe a model of earnings and earnings growth and I demonstrate how this model may be used to obtain estimates of the expected rate of return on equity capital. These estimates are compared with estimates of the expected rate of return implied by commonly used heuristics—*viz.*, the PEG ratio and the PE ratio. Proponents of the PEG ratio (which is the price-earnings [PE] ratio divided by the short-term earnings growth rate) argue that this ratio takes account of differences in short-run earnings growth, providing a ranking that is superior to the ranking based on PE ratios. But even though the PEG ratio may provide an improvement over the PE ratio, it is arguably still too simplistic because it implicitly assumes that the short-run growth forecast also captures the long-run future. I provide a means of simultaneously estimating the expected rate of return and the rate of change in abnormal growth in earnings beyond the (short) forecast horizon—thereby refining the PEG ratio ranking. The method may also be used by researchers interested in determining the effects of various factors (such as disclosure quality, cross-listing, etc.) on the cost of equity capital. Although the correlation between the refined estimates and estimates of the expected rate of return implied by the PEG ratio is high, supporting the use of the PEG ratio as a parsimonious way to rank stocks, the estimates of the expected rate of return based on the PEG ratio are biased downward. This correlation is much lower and the downward bias is much larger for estimates of the expected rate of return based on the PE ratio. I provide evidence that stocks for which the downward bias is higher can be identified *a priori*.

Keywords: *PE ratio; PEG ratio; earnings forecasts; earnings growth; cost of capital.*

Data Availability: *All analyses are based on publicly available data.*

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I. INTRODUCTION

I describe a model of earnings and earnings growth and I demonstrate how the model may be used to obtain estimates of the expected rate of return on equity capital. These estimates are the rates of return implied by current prices and forecasts of future payoffs (earnings and earnings growth), analogous to internal rates of return calculated from the market price of a bond and the coupon payments. I compare these estimates of the expected rate of return with those implied by commonly used heuristics—*viz*., the PEG ratio and the PE ratio.

The PEG ratio (which is the price-earnings [PE] ratio divided by the short-term earnings growth rate) has become a popular means of combining prices and forecasts of earnings and earnings growth into a ratio that is used as a basis for stock recommendations (implicitly for comparing expected rates of return). Proponents of the PEG ratio argue that this ratio takes account of differences in short-run earnings growth and, thus, it provides a ranking that is superior to the ranking based on PE ratios.[1] But even though the PEG ratio may provide an improvement over the PE ratio, it is arguably still too simplistic because it implicitly assumes that the short-run growth forecast also captures the long-run future. I provide a means of simultaneously estimating the expected rate of return and the rate of change in abnormal growth in earnings beyond the (short) forecast horizon—thereby refining the PEG ratio ranking.[2] The method may also be used by researchers interested in determining the effects of various factors (such as disclosure quality, cross-listing, etc.) on the cost of equity capital.

My model is based on Ohlson and Juettner-Nauroth (2000). I isolate the respective roles of (1) forecasts of next period's accounting earnings, (2) forecasts of short-run growth in accounting earnings from this base, and (3) expected growth in accounting earnings beyond the short forecast horizon. I show how the difference between accounting earnings and economic earnings characterizes the role of accounting earnings in valuation.[3] In short, (1) if the forecast of next period's accounting earnings is equal to economic earnings, then these earnings are, by definition, sufficient for valuation and the expected rate of return is equal to the inverse of the price to expected earnings (PE) ratio; (2) if the forecast of next period's accounting earnings is not equal to economic earnings but the abnormal growth in accounting earnings is constant in perpetuity, then these forecasts are sufficient for valuation and the expected rate of return is equal to the square root of the inverse of the 100 times the PEG ratio; and (3) if the next period forecast of accounting earnings is not equal to economic earnings and the abnormal growth in accounting earnings is not expected to be constant in perpetuity, then I derive a third growth variable that may be used to adjust these forecasts to obtain valuation sufficiency (and an estimate of the expected rate of return). In other words, short-run forecasts of abnormal growth in accounting earnings and a variable that captures *change* in this growth beyond the forecast horizon may be used to adjust for the fact that analysts forecast accounting earnings rather than economic earnings.

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[1] An argument for the use of the PE ratio as a basis for stock recommendations is that, *ceteris paribus*, high (low) PE implies low (high) expected rate of return, supporting a sell (buy) recommendation. The essence of the argument for the use of the PEG ratio as a basis for stock recommendations is that, *ceteris paribus*, high (low) PEG implies that the PE ratio is high (low) relative to the expected rate of growth in earnings suggesting that the future prospects are expected to worsen (improve), implicitly the expected rate of return is low (high), supporting a sell (buy) recommendation. I elaborate on the use of the PEG ratio in the next section of the paper.

[2] Abnormal growth in earnings is the forecast of two-period-ahead cum-dividend earnings minus "1-plus" the expected rate of return multiplied by the forecast of one-period-ahead earnings. In other words, abnormal growth in accounting earnings is growth over and above the expected growth (conditional on the expected rate of return). This variable is discussed in detail in Section III.

[3] Economic earnings are defined in this paper as the product of the expected rate of return and beginning-of-period price. Analysts forecast GAAP earnings after adjusting for one-time items (see Bhattacharya et al. [2003] for a discussion of these adjustments). Since these are the accounting earnings forecasts used in this paper, the analyses show how the difference between I/B/E/S forecasts of earnings and economic earnings characterizes the role of the I/B/E/S forecasts in valuation. The model and the arguments could be applied to any forecasts of earnings (including GAAP earnings).

The three elements of the earnings forecast—(1), (2), and (3), above—are at the core of the empirical analyses that focus on the effects on the estimate of the expected rate of return of progressively relaxing restrictive assumptions implicit in simple, and commonly used, earnings-based valuation heuristics. The internal rate of return implied by prices and all three elements of the earnings forecast (forecasts of next period earnings, short-run earnings growth, and change in this growth rate beyond the forecast horizon) is compared with (a) the estimate of the expected rate of return that assumes no change in the earnings growth rate beyond the forecast horizon (that is, the expected rate of return implied by the PEG ratio) and (b) the expected rate of return that is implied by the PE ratio (that is, via the implicit assumption that forecasted next period earnings are equal to economic earnings).

The key elements of my model are very similar to the key elements of the residual income valuation model that has been used to obtain estimates of the expected rate of return in a number of recent studies in the accounting and finance literature.[4] Ohlson (2001) has pointed out that a possible limitation of these studies is that many of them rely on the clean-surplus assumption in the forecast of future book values and this assumption rarely holds as a practical matter.[5] Although the residual income valuation model is becoming more widely used on Wall Street,[6] analysts' reports still pervasively focus on forecasts of earnings and earnings growth rather than book value and the forecasts of book value growth that are implicit in the residual income valuation model.[7] In other words, analysts' reports have an earnings (or income statement) focus rather than a book value (or balance sheet) focus. My model and the empirical analyses in this paper also focus on earnings.

Botosan and Plumlee (2002), Easton and Monahan (2003), and Gode and Mohanram (2003) compare estimates of the expected rate of return obtained from the residual income model with estimates obtained from models based on Ohlson and Juettner-Nauroth (2000).

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[4] See, for example, Botosan (1997), Botosan and Plumlee (2002), Baginski and Wahlen (2003), Claus and Thomas (2001), Easton et al. (2002), and Gebhardt et al. (2001). The three elements—(1), (2), and (3)—have direct analogues in the implementation of the residual income valuation model: (1) if book value is equal to market value, then book value (that is, the bottom line of the balance sheet) is sufficient for valuation; (2) if short-run forecasts of accounting earnings are such that market value is equal to book value at the end of the short forecast horizon, then book value and these forecasts are sufficient for valuation; and (3) if short-run forecasts of accounting earnings are not such that market value is equal to book value at the end of the short forecast horizon, then a forecast of growth in residual income beyond the forecast horizon is sufficient for valuation.

[5] Ohlson (2001) gives the following reasons: (1) clean surplus rarely holds on a per share basis, (2) even on a total equity basis, the residual income valuation model does not hold if buying shares is a positive net present value project from the perspective of the new shareholders, and (3) many accounting rules violate the clean-surplus relation. Further, a number of recent studies (for example, Abarbanell and Lehavy 2002; Bhattacharya et al. 2003; Johnson and Schwartz 2003) have shown that there are significant differences between GAAP earnings and the earnings that are forecasted by analysts and these forecasts inevitably do not reflect comprehensive income. It follows that these forecasts cannot be used to obtain meaningful forecasts of book value (and, hence, residual income).

[6] See, for example, Morgan Stanley Dean Witter, *Scale and Scope Tip the Balance in EMS Valuations*, (2001) and HSBC, "Pulling Power" (2001).

[7] For example, Bloomberg L.P. generally lists earnings forecasts for the next year and forecasts of earnings growth for the short-run future on its website http://www.bloomberg.com/analysis/research.html. Similarly, I/B/E/S provides forecasts of earnings for the current year, for the next year, and for the short-run future. Value Line provides forecasts for the current year, for the next year, and a forecast of average earnings for the three years thereafter, implicitly providing forecasts of short-run expected earnings growth.

The basis for the comparison in Botosan and Plumlee (2002) and Gode and Mohanram (2003) is the relation between the estimates of the expected rate of return and various measures of risk. Easton and Monahan (2003) use realized returns as their basis for comparison. Rather than drawing comparisons across particular implementations of the two models, I emphasize the effect of progressively relaxing restrictive assumptions in the implementation of the earnings and earnings growth model—particularly the restrictive assumptions implicit in the use of the PEG ratio and the PE ratio as bases for stock recommendations.

I demonstrate the method for simultaneously estimating the expected rate of return and the long-run change in abnormal growth in earnings that are implied by prices and analysts' short-term earnings forecasts using I/B/E/S forecasts over the years 1981 to 1999. I estimate the expected rate of return and the long-run change in abnormal growth in earnings for 1,499 portfolios of 20 stocks formed annually, based on the magnitude of the PEG ratio. The mean estimate of the expected rate of return is 13 percent with a standard deviation of 3.9 percent. The mean estimate of the long-run change in abnormal growth in earnings is 2.9 percent with a standard deviation of 3.0 percent. In other words, an investment at current market prices with I/B/E/S expectations of earnings and short-run earnings growth and an implicit expectation of long-run change in abnormal growth in earnings of 2.9 percent, implies an internal rate of return of 13 percent.

The Spearman correlation between the PEG ratio and the refined estimate of the expected rate of return is –0.90. This large negative correlation supports the use of the PEG ratio heuristic as a simple basis for stock recommendations that implicitly reflect the ranking (rather than the magnitude) of expected returns on portfolios of stocks. However, estimates of the magnitude of the expected return that are based on the PEG ratio heuristic are generally biased downward (the mean downward bias is 1.7 percent with a standard deviation of 1.4 percent).[8] The downward bias is greater for firms with lower short-term earnings growth rates, higher PEs, and higher ratios of price-to-book value, while the bias is lower for larger firms and firms with a higher standard deviation of returns. Furthermore, the bias in the estimates of the expected rate of return is very large for some stocks (10.96 percent for one of the portfolios of stocks).

Consistent with analysts' conjectures that the PEG ratio is better than the PE ratio as a means of ranking stocks, the correlation between the PE ratio and the refined estimate of the expected rate of return is much lower (–0.48) than the correlation between the PEG ratio and the refined measure (–0.90). In addition, the estimate of the expected rate of return based on the PE ratio is much more downward-biased (mean bias of 4.6 percent).

The paper proceeds as follows. I begin with a discussion of the use of the PEG ratio as a means of comparing stocks. Next, I develop the model of the relation between prices, forecasts of earnings, and forecasts of earnings growth. The PEG ratio heuristic and, in turn, the PE ratio heuristic are shown to be special cases of this model. The model is then used as the basis for the development of the empirical procedure that simultaneously estimates the expected rate of return and the expected long-run change in abnormal growth in earnings. The remainder of the paper is a demonstration of the use of this method and a comparison of the estimates of the expected rate of return from this method with the estimates based on the PEG ratio and estimates based on the PE ratio. I investigate the characteristics of firms for which the differences between the estimates of the expected rate of return are highest.

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[8] That is, the internal rate of return is underestimated by 1.7 percent.

II. THE PEG RATIO

The PEG ratio is equal to the price-earnings ratio divided by an earnings growth rate. Analysts differ in their choice of the form of the price-earnings ratio (that is, price-to-trailing earnings or price-to-forward earnings) and in their choice of the earnings growth rate (ranging from a one-year historical growth rate to an average expected annual growth rate estimated for several years).

Numerous articles in the popular press describe the pervasiveness of the use of the PEG ratio as a basis for stock recommendations.[9] The PEG ratio and its use in valuation is described on http://www.fool.com/School/TheFoolRatio.htm and is advocated by well-known Wall Street analyst, Peter Lynch, in his book *One Up On Wall Street* (Lynch 2000). The arguments for the use of the PEG ratio vary considerably but the essence of these arguments may be summarized as follows.

Use of the price-to-forward earnings (PE) ratio as a basis for stock recommendations relies on the notion that, *ceteris paribus,* high (low) PE implies a low (high) expected rate of return, supporting a sell (buy) recommendation. However, next period's earnings may not be indicative of the future stream of earnings and as Lynch (2000, 199) observes:

> A company, say, with an [earnings] growth rate of 12 percent a year...and a p/e ratio of 6 is a very attractive prospect. On the other hand, a company with a growth rate of 6 percent a year and a p/e ratio of 12 is an unattractive prospect and headed for a comedown.

He goes on to say:

> The p/e ratio of any company that's fairly priced will equal its [earnings] growth rate...In general, a p/e ratio that's half the growth rate is very positive, and one that's twice the growth rate is very negative. We use this measure all the time in analyzing stocks for mutual funds.

This comparison of the PE ratio and the earnings growth rate as a basis for stock recommendations is captured in the PEG ratio. Consistent with Lynch's (2000) argument, a stock is fairly priced if its PEG ratio is equal to 1 and analysts would recommend holding the stock. A PEG ratio considerably greater (less) than 1 would support a sell (buy) recommendation. To summarize, the essence of the argument for the use of the PEG ratio is that, *ceteris paribus,* high (low) PEG implies that the PE ratio is high (low) relative to the expected rate of growth in earnings, suggesting that the future prospects are expected to worsen (improve), implicitly the expected rate of return is low (high), supporting a sell (buy) recommendation.

Despite the pervasive use of the PEG ratio, its proponents do not provide a model that is based on fundamental valuation theory. In the next section, I describe a simple model based on the dividend capitalization formula and the concept of economic earnings (that is, price multiplied by the expected rate of return), which shows that under certain restrictive assumptions the PEG ratio multiplied by 100 is equal to the inverse of the square of the expected rate of return.[10] This model supports the use of the PEG ratio as a means of ranking stocks—higher PEG ratios imply that the market expects a lower rate of return.

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[9] For example, Barbash (1998), Braham (2001), Badenhausen (2002), Fuerbringer (2000), Kadlec (2001), Brown (2001), and Siegel (2001). Financial websites, (for example, http://www.stockselector.com and http://www.fool.com/pegulator) often report PEG ratios and rely on these ratios as a primary basis for stock recommendations. Bradshaw (2001, 2002) further elaborates on the extensive use of the PEG ratio in analysts' reports.

[10] It is interesting to note that a "normal" PEG ratio of unity corresponds to an expected rate of return of 10 percent, which is the same rate as suggested by Black (1980) in his use of 10 as a "normal" PE ratio.

The model may also be used to provide a foundation for the analysts' intuition that a stock is fairly priced if its PEG ratio is equal to 1. The argument is as follows. If forecasted earnings for every future period are economic earnings, then the PE ratio is equal to the expected rate of return and earnings will continue beyond the next period to grow at the expected rate of return. That is, the PE ratio is equal to the earnings growth rate, the PEG ratio is equal to 1, and the stock is fairly priced. Central to this argument, however, is the equality of forecasted earnings and economic earnings. I will elaborate on this in the next section of the paper.

III. A MODEL OF THE PEG RATIO

A key element of the model is recognition of the central role of short-term forecasts of earnings in valuation. I isolate the respective roles of (1) forecasts of next period's accounting earnings, (2) forecasts of accounting earnings two periods ahead, and (3) expected accounting earnings beyond the two-year forecast horizon. I show how the difference between accounting earnings and economic earnings characterizes the role of accounting earnings in valuation.

I begin with the no arbitrage assumption:

$$P_0 = (1 + r)^{-1}[P_1 + dps_1] \tag{1}$$

where:

P_0 = current, date $t = 0$, price per share;
P_1 = expected, date $t = 1$, price per share;
dps_1 = expected dividends per share, at date $t = 1$; and
r = expected rate of return and $r > 0$ is a fixed constant.[11]

The central valuation role of forecasts of next period's accounting earnings is examined by adding (and subtracting) capitalized expected accounting earnings, eps_1/r to Equation (1) and focusing on the term that remains when capitalized accounting earnings is separately identified. Adding and subtracting capitalized accounting earnings yields:

$$P_0 = eps_1/r - [eps_1/r - (1 + r)^{-1}(P_1 + dps_1)]. \tag{2}$$

If expected accounting earnings eps_1 is equal to economic earnings (which may be defined as rP_0), then the term in brackets must be equal to zero—in other words, next period's expected earnings are sufficient for valuation. However, if eps_1 does not equal economic earnings, then valuation based on accounting earnings requires forecasts beyond the next period.

The role of two-period-ahead forecasts of accounting earnings, may be seen by rewriting Equation (2):

$$P_1 = eps_2/r - [eps_2/r - (1 + r)^{-1}(P_2 + dps_2)]. \tag{3}$$

Substituting Equation (3) into Equation (2) yields:

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[11] Although Equation (1) may be viewed as a succinct statement of the no arbitrage assumption, it may also be viewed as a definition of expected return (r) even in a world where arbitrage opportunities exist. Even under this alternate view, the analysis that follows calculates the internal rate of return based on prevailing prices and expectations of expected pay-offs.

$$P_0 = eps_1/r + r^{-1}(1 + r)^{-1}agr_1 + (1 + r)^{-2}r^{-1}[rdps_2 - (1 + r)eps_2] + (1 + r)^{-2}P_2 \tag{4}$$

where:

$$agr_1 = [eps_2 + rdps_1 - (1 + r)eps_1]. \tag{5}$$

agr_1 is expected abnormal growth in accounting earnings insofar as it is expected (period 2) cum-dividend accounting earnings ($ceps_2 = eps_1 + rdps_1$) less the normal accounting earnings that would be expected given earnings of period 1 (that is, $(1 + r)eps_1$). This abnormal growth in earnings reflects the effects of generally accepted accounting practices that lead to a divergence of accounting earnings from economic earnings. To see this, note that if eps_1 and eps_2 were equal to economic earnings, then agr_1 would be zero and the ratio of expected earnings-to-price would be equal to the expected rate of return.

For example, consider Microsoft, which was trading at a price per share of $75 at the end of its fiscal year (June 30) 2001 and was not expected to pay dividends for the foreseeable future. If Microsoft's expected rate of return was 10 percent, then its expected economic earnings for 2002 and 2003 would have been $7.50 and $8.25, respectively. If accounting earnings (eps_1 and eps_2) were equal to economic earnings in these years, then $agr_1 = \$8.25 - 1.1(\$7.50) = 0$ and eps_1 would be sufficient for valuation (that is, $\$75 = \$7.50/0.1$). Yet analysts were forecasting accounting earnings for 2002 and 2003 of $1.90 and $2.15, respectively, so that $agr_1 = \$2.15 - 1.1(\$1.90) = \$0.06$. In other words, the difference between expected accounting earnings and expected economic earnings in 2002 and 2003 implies accounting earnings growth of 6 cents more than the cost of capital. I will return to this example.

The valuation role of expected accounting earnings beyond the two-year forecast horizon may be seen by recursively substituting for P_2, P_3, P_4, etc., in Equation (5) to yield:

$$P_t = eps_1/r + r^{-1} \sum_{t=1}^{\infty} (1 + r)^{-1}agr_t. \tag{6}$$

That is, Equation (6) shows that the present value of the agr_t-sequence explains the difference between price and capitalized expected earnings.[12]

Equation (6) may be modified to accommodate a finite forecast horizon by defining a perpetual rate of change in abnormal growth in earnings (Δagr) beyond the forecast horizon. In particular, if earnings forecasts are available for two periods, Equation (6) may be rewritten as:

$$P_0 = eps_1/r + agr_1/(r(r - \Delta agr)) \tag{7}$$

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[12] A similar relation is derived in Ohlson and Juettner-Nauroth (2000). The analogy to the residual income valuation model is apparent from Equation (6). Just as in the residual income model where future residual income is nonzero if price is not equal to book value (that is, future residual income represents the future earnings adjustment—growth in book value—to recognize the difference between price and book value) future abnormal growth in earnings adjusts for the difference between next period's accounting earnings and next period's economic earnings.

where:

$$\Delta agr = (agr_{t+1}/agr_t) - 1. \tag{8}$$

In Equation (7), Δagr is the unique perpetual rate of change in abnormal growth in earnings, which, if known, would permit the estimation of the internal rate of return implied by the current price and forecasts of eps_1 and/or eps_2. If eps_1 and eps_2 are equal to economic earnings, then the expected rate of return is equal to the ratio of forecasted earnings-to-current price. If eps_1 and/or eps_2 are not equal to economic earnings, then agr_1 will be nonzero and Δagr captures the future long-run change in abnormal growth in accounting earnings to adjust for this difference between accounting and economic earnings.

Returning to the Microsoft example, the estimate of Δagr that equates the price of \$75 and the forecasts of accounting earnings is 8.9 percent.[13] In other words, 8.9 percent is the geometric average rate at which the abnormal growth in earnings of 6 cents will increase as accounting earnings eventually "correct" for the short-run difference between accounting and economic earnings in the two-year forecast horizon.[14] The difference between short-run forecasts of accounting earnings (\$1.90 and \$2.15) and expected economic earnings (\$7.50 and \$8.25) determines the abnormal growth in earnings agr_1 "base." The abnormal growth in earnings will change from this base at a geometric average rate Δagr of 8.9 percent in the future. As an illustration of the relation between agr_1 and Δagr, suppose that the forecast of earnings for 2003 includes a nonrecurring item of −\$0.50. The forecast of earnings if this nonrecurring item had been removed would be \$2.65 instead of \$2.15. This higher earnings forecast implies a much higher agr_1 (\$0.56) and a much lower Δagr (zero). That is, accounting earnings growth of \$0.56 greater than the cost of capital in perpetuity is sufficient to explain the price of \$75. In other words, this perpetual growth in accounting earnings is sufficient for accounting earnings to eventually correct for the difference between short-run forecasts of accounting earnings (\$1.90 and \$2.65) and expected economic earnings (\$7.50 and \$8.25).

The PEG ratio, which is equal to the PE ratio (that is, P_0/eps_1) divided by the short-term rate of growth in earnings expressed as a percentage (that is, $100^*(eps_2 - eps_1)/eps_1$), is a special case of this model. This, and a closely related special case, may be used to obtain estimates of r. Details follow.

Consider the special case $\Delta agr = 0$.[15] That is, $agr_1 = agr_2 = ,...,$ and the next period's expected abnormal growth in earnings provides an unbiased estimate of all subsequent periods' abnormal growth in earnings. From Equation (7) it can be seen that this special case may be written:

$$P_0 = [eps_2 + rdps_2 - eps_1]/r^2 \tag{9}$$

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[13] This estimate is obtained by recognizing that, for this Microsoft example, the only unknown term in Equation (7) is Δagr.

[14] This growth reflects the attribute of accounting that differences between accounting earnings and economic earnings in any one period must be captured in accounting earnings of another period. (See Easton et al. [1992] for an elaboration of this point.)

[15] There will be cases where the assumption $\Delta agr = 0$ is a very reasonable first approximation. For example, consider Honda Motor Company (HMC), which was trading at a price per share of \$80 at the end of its fiscal year (March 31) 2001. Analysts were forecasting earnings for 2002 and 2003 of \$5.50 and \$6.25, respectively, and dividends of \$0.50. If Honda's expected rate of return is 10 percent, then $agr_1 = \$6.25 + 0.1(\$0.50) - (1.1)\$5.50 = 0.25$. In this example, $P_0 = [eps_1/r] + [agr_1/r^2] = [\$5.50/0.1] + [0.25/0.01]$ and $\Delta agr = 0$.

and

$$r = \sqrt{(eps_2 + rdps_1 - eps_1)/P_0}. \tag{10}$$

That is:

$$r^2 - r(dps_1/P_0) - (eps_2 - eps_1)/P_0 = 0. \tag{11}$$

In the subsequent empirical analyses, observed prices and forecasts of earnings and dividends are used to obtain an estimate of r as the solution to this quadratic equation.[16] I constrain $eps_2 \geq eps_1 > 0$ so that solution to Equation (11) has two real roots, one of which is positive.[17] I use the positive root since the negative root is meaningless.

As a second special case, assume $\Delta agr = 0$ and $dps_1 = 0$, then the right-hand-side of (10) is the square root of the inverse of 100 multiplied by the PEG ratio:[18]

$$r = \sqrt{(eps_2 - eps_1)/P_0} \tag{12}$$

Firm-specific estimates of the expected rate of return derived from Equations (11) and (12) are provided in the empirical analyses. I will refer to the estimate of the expected rate of return from Equation (12) as the estimate based on the PEG ratio (r_{PEG}).[19] The estimate from Equation (11) (r_{MPEG}) will be referred to as the estimate based on the modified PEG ratio where the modification is the inclusion of expected dividends in the estimate of short-term growth.

Finally, consider the third special case where $agr_1 = 0$. In this case, it is evident from Equation (7) that the expected rate of return will equal eps_1/P_0. I refer to this estimate as r_{PE}.

IV. SIMULTANEOUS ESTIMATION OF THE EXPECTED RATE OF RETURN AND LONG-RUN CHANGE IN ABNORMAL GROWTH IN EARNINGS

In this section, I derive a method for simultaneous estimation of the expected rate of return ($r_{\Delta agr}$) and long-run change in the rate of abnormal growth in earnings (Δagr) for a portfolio of stocks.[20] That is, I relax the assumption that $\Delta agr = 0$ implicit in the PEG ratio estimation procedure (above). In subsequent sections, I compare estimates of the expected rate of return obtained with and without this assumption. These estimates facilitate evaluation of the effectiveness of the PEG ratio as a method for ranking stocks.

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[16] In the Honda example, $P_0 = \$80$, $eps_1 = \$5.50$, $dps_1 = 0.50$, and $eps_2 = \$6.25$ so that the implied expected rate of return is 10 percent. Without the agr/r^2 correction to eps_1/r (see footnote 15), the price to expected earnings ratio is 14.54 with an implied expected rate of return of 6.9 percent.

[17] The empirical analyses rely on I/B/E/S forecasts of earnings two years ahead as a proxy for eps_2 and I/B/E/S forecast of earnings one year ahead as a proxy for eps_1. The constraint $eps_2 \geq eps_1$ leads to deletion of 0.7 percent of the observations. Since I/B/E/S do not provide forecasts of dividends, I assume that $dps_1 = dps_0$.

[18] $(eps_2 - eps_1)/P_0 = [(eps_2 - eps_1)/eps_1]/PE = 1/[PEG*100]$.

[19] For the Microsoft example, $r_{PEG} = \sqrt{(\$2.15 - \$1.90)/\$75} = 5.7\%$, and for the Honda example, $r_{PEG} = \sqrt{(\$6.25 - \$5.50)/\$80} = 9.68\%$. As the proponents of the PEG ratio point out, these estimates of the expected rate of return are closer to the true expected rate of return of (10 percent) than the rate implied by the assumption that $agr_1 = 0$ (that is, 2.53 percent and 6.9 percent).

[20] The method parallels that in Easton et al. (2000) who use the residual income model to motivate a regression of forecasted return on equity on the price-to-book ratio and use the coefficients from this regression to obtain simultaneous estimates of the implied expected rate of return and the growth in residual income beyond the forecast horizon.

Expanding Equation (7) using the definition of agr_1 from Equation (5) and re-arranging yields:

$$ceps_2/P_0 = \gamma_0 + \gamma_1 eps_1/P_0 \tag{13}$$

where $\gamma_0 = r_{\Delta agr}(r_{\Delta agr} - \Delta agr)$, $\gamma_1 = (1 + \Delta agr)$ and $ceps_2$ is the forecast of two-period-ahead cum-dividend earnings. The expected rate of return ($r_{\Delta agr}$) is the rate of return that is implied by current prices (P_0), forecasts of earnings (eps_1 and eps_2), and the implied long-run change in the rate of abnormal growth in earnings (Δagr).

Although the analysis in Section III is for an individual firm, we can write Equation (13) as the following linear relation for each firm j:

$$ceps_{j2}/P_{j0} = \gamma_{j0} + \gamma_{j1} eps_{j1}/P_{j0}. \tag{14}$$

The linear relation between $ceps_{j2}/P_{j0}$ and eps_{j1}/P_{j0} in Equation (14) suggests that the average (portfolio) expected rate of return ($r_{\Delta agr}$) and the average rate of change in abnormal growth in earnings (Δagr) may be estimated from the intercept and the slope coefficients from a linear regression of $ceps_{j2}/P_{j0}$ on eps_{j1}/P_{j0} for any portfolio of $j = 1,\ldots,J$ stocks.

I run the regression:

$$ceps_{j2}/P_{j0} = \gamma_0 + \gamma_1 eps_{j1}/P_{j0} + e_{j0}. \tag{15}$$

Notice that there is no error term in Equation (14). The error term e_{j0} in Regression (15) arises because of the firm-specific random component of the coefficients γ_{j0} and γ_{j1}.[21] The estimates of the coefficients γ_0 and γ_1 may be regarded as the mean of the firm-specific coefficients. It follows that the $r_{\Delta agr}$ and Δagr implied by these estimates are the estimates for the portfolio of J firms.

Regression (15) may be used to obtain an estimate of the expected rate of return implied by any set of prices and forecasts of earnings. For example, researchers interested in determining the effect of earnings quality on the cost of capital could partition firms into two portfolios—those with high-quality disclosure and those with low-quality disclosure—and then compare the estimates of the cost of capital across the two portfolios.

I run Regression (15) for portfolios of stocks formed on the magnitude of the PEG ratio to obtain estimates of $r_{\Delta agr}$ and Δagr for these portfolios.[22] Since the stocks in each portfolio have virtually the same PEG ratios, they also have virtually the same r_{PEG}. This facilitates a clear comparison of r_{PEG} and the refined estimates of the expected rate of return ($r_{\Delta agr}$), which take long-run change in abnormal growth in earnings (Δagr) into account. An advantage of forming portfolios on the basis of the PEG ratio is that the R^2 when Regression (15) is run for the portfolio is very high (almost 1). The reason is as follows: $ceps_2/P_0 = 1/PEG\text{-}ratio + rdps_1/P_0 + eps_1/P_0$ and, since portfolios are formed on the basis of PEG ratios, the variance of $1/PEG\text{-}ratio$ (within the portfolio) will be very small relative to the variance of eps_1/P_0. Also, the variance of $rdps_1/P_0$ will be small relative to the variance of eps_1/P_0. Thus, the R^2 from a regression of $ceps_2/P_0$ on eps_1/P_0 will be high and the estimates of $r_{\Delta agr}$ will be very precise. This method also mitigates the classic "errors in variables" problem (see Maddala 1977, 292–293) that arises if one argues that both the dependent variable and the independent variable in Regression (15) contain measurement error. In this situation, $r_{\Delta agr}$ and Δagr lie within the range that has bounds specified by Regression (15) and the "reverse" regression of eps_{j1}/P_{j0} on $ceps_{j2}/P_{j0}$. The range is a decreasing function of the regression R^2. Since I select a sample where the regression R^2 is very high (almost 1), the range is very small.[23]

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[21] Since $e_{j0} = \gamma_{j0} - \gamma_0 + (\gamma_{j1} - \gamma_1)eps_{j1}/P_{j0}$, the error term is heteroscedastic, and White (1980) corrections should be made to the standard errors.

[22] Analyses were also repeated on portfolios matched on SIC code and size with qualitatively very similar results. Details of the portfolio formation procedures are provided in Section V.

Recall that the aims of the paper are (1) to develop and demonstrate a method for estimating the expected rate of return and the expected long-run change in abnormal growth in earnings that are implied by market prices and analysts' forecasts of earnings, and (2) to compare this estimate of the expected rate of return with estimates based on analysts' heuristics that do not take long-run growth (that is, the PEG ratio) and short-run growth (that is, the PE ratio) into account. With this in mind, $ceps_{j2}$ is obtained by using I/B/E/S analysts' forecasts as the measure of eps_2 and assuming $dps_1 = dps_0$.[24] The estimate of eps_1 is also obtained from I/B/E/S. Of course, the method could be used to convert any forecasts into an implied expected rate of return.

V. DATA AND SAMPLE SELECTION

Price at fiscal year-end, dividends, and number of shares outstanding are obtained from the 1999 Compustat annual primary, secondary, tertiary, and full coverage research files.[25] Earnings forecasts are derived from the summary 2000 I/B/E/S tape. I determine median forecasts from the available analysts' forecasts on the I/B/E/S file released on the third Thursday of December. I include only firms with December fiscal year-end.[26] For observations in 1995, for example, the December 1995 forecasts became available on 21st December. These data include forecasts for a fiscal year ending just ten days later (that is, December 31, 1995), an earnings forecast for the fiscal year ending December 31, 1996 (that is, eps_1), and a forecast of growth in earnings for the subsequent three years. This three-year growth rate is used to obtain estimates of eps_2 and it is also used as the estimate of the short-term earnings growth rate used in the denominator of the PEG ratio.[27] Firms are ranked each year on the magnitude of their PEG ratio and formed into a total of 1,499 portfolios of 22 firms.[28]

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[23] I thank Carter Hill and Andrew Christie for bringing this to my attention. This method does not address bias in the analysts' forecasts. If analysts' forecasts are on average optimistic, then estimates of the expected rate of return will also be optimistic.

[24] Calculation of $ceps_{j2}$ requires an estimate of $r_{\Delta agr}$ yet I am using $ceps_{j2}$ to estimate $r_{\Delta agr}$. To overcome this apparent circularity, I use the following iterative procedure. I begin by assuming that the displacement of future earnings due to the payment of dividends is 12 percent of the dividend payment based on the assumption that, if these dividends had been retained in the firm, they would have earned roughly the historical market return. Then I use the following iterative procedure for dealing with the fact that calculation of Δagr is based on $r_{\Delta agr}$ rather than an assumed rate—in this case 12 percent. After calculating $r_{\Delta agr}$ and Δagr based on this assumption, I then recalculate Δagr using the estimated $r_{\Delta agr}$. I then repeat the regression analysis with this revised estimate of $r_{\Delta agr}$ to obtain revised estimates of $r_{\Delta agr}$ and Δagr. I repeat this procedure until the revision in the estimate of Δagr leads to no change in the estimates of $r_{\Delta agr}$ and Δagr. Convergence (defined as no change in $r_{\Delta agr}$ and Δagr) generally occurs after just two iterations. The estimates of $r_{\Delta agr}$ and Δagr are not sensitive to assumed starting rates for $r_{\Delta agr}$ between 0 and 30 percent.

[25] All analyses were also done using I/B/E/S prices instead of Compustat prices. The results are qualitatively similar.

[26] All analyses in the paper have been repeated using the means of available forecasts rather than the medians. The results are qualitatively very similar. I deleted firms with non-December year-ends so that the market implied discount rate and growth rate are estimated at the same point in time for each firm-year observation.

[27] In this example, firm-year observations with a negative forecast of earnings for 1996 are deleted because growth from this negative base is not meaningful in this context. The number of observations deleted for this reason in each of the years 1981 to 1999 was 11, 22, 14, 30, 35, 58, 55, 47, 50, 73, 72, 88, 109, 121, 134, 287, 322, 363, and 487, respectively.

VI. ESTIMATES OF THE EXPECTED RATE OF RETURN AND LONG-RUN CHANGE IN ABNORMAL GROWTH IN EARNINGS

Descriptive statistics and the results from estimation of Regression (15) are presented in Table 1. The number of portfolios increases from a low of 40 in 1981 to a high of 132 in 1997, with 130 and 109 observations in 1998 and 1999, respectively. The median of the estimates of the intercept γ_0 ranges from a low of 0.0097 in 1997 to a high of 0.0189 in 1981.[29] The median of the estimates of the slope coefficient γ_1 ranges from a low of 0.9999 in 1982 to a high of 1.0609 in 1985. The median adjusted R^2 is very high (almost 1) in all years.

The median portfolio estimate of $r_{\Delta agr}$ ranges from 11.6 percent in 1983 to 15.6 percent in 1984.[30] These estimates are similar to the realized historic return on listed stocks—for example, the average return on the Standard & Poor's 500 Index over the years 1926 to 1999 was 13 percent.[31] The ten-year U.S. government bond rates at the end of each year are also included in the last column of Table 1.[32] The market equity risk premium (that is, the difference between $r_{\Delta agr}$ and the U.S. government bond rate) is positive in most years: it is low in the early years when the government bond rate is very high.[33] The average equity risk premium is 4.8 percent. This estimate is similar to the estimate of 5.2 percent obtained by Easton et al. (2002) using earnings forecasts and the residual income valuation model over the same period.[34] Both of these estimates are similar to the estimate based on historical earnings data (5 percent) in Fama and French (2002). The median estimate of Δagr ranges from 0 percent in 1982 to 6.1 percent in 1985.

The estimates of r_{PEG} and r_{MPEG} are also included in Table 1. The median estimates of r_{PEG} are lower than r_{MPEG} in every year other than 1981, 1982, 1983, and 1997 when they are the same. Overall, the inclusion of expected dividends in the calculation of the expected rate of return (r_{MPEG}) increases the median estimate of the expected rate of return from 11.3 percent to 11.9 percent. In other words, excluding expected (next year) dividends from the estimation of the expected rate of return biases the estimate downward.

A comparison of $r_{\Delta agr}$ and r_{PEG} reveals that the restrictions that expected (next year) dividends are zero and $\Delta agr = 0$ bias the estimate of the expected rate of return downward. This downward bias is consistent with the median positive estimate of Δagr (2.9 percent). The median estimate of $r_{\Delta agr}$ is higher than the median estimate of r_{PEG} in every year other than 1982 and 1983 when they are the same (and the median Δagr is 0). The difference between the median of all estimates of $r_{\Delta agr}$ and the median of all estimates of r_{PEG} is 1.7 percent. An example of the possible effect of failing to take change in abnormal growth in

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[28] In order to reduce the effect of outliers in the estimation of $r_{\Delta agr}$ and Δagr, the observations with the highest eps_{j1}/P_{j0} are removed. Inclusion of the outliers leads to a wider dispersion in these estimates than is shown in Table 1, but the rank correlations in the subsequent tables are not affected.

[29] I report only medians in the tables. Means occasionally differ considerably from medians due to a few outliers.

[30] These estimates are similar in magnitude to those estimated using the residual income valuation model in Easton et al. (2002).

[31] Ibbotson (1999).

[32] The validity of comparisons with the T-Bond rate is limited by the fact that we are calculating an r for the infinite horizon future while the T-Bond rate is for a finite ten-year horizon.

[33] The relatively low estimated premium in the early years of the data may be partially explained by the fact that I/B/E/S tended to provide forecasts for larger firms during this period.

[34] The average equity premium of 4.8 percent is not reported in the table. This average premium is calculated by taking the difference between the median $r_{\Delta agr}$ and the U.S. government bond rate for each year and then averaging over the 19 years.

TABLE 1
Sample Description, Selected Statistics from Estimation of Regression (15), and Annual Estimates of the Expected Rate of Return and Long-Run Change in Abnormal Growth in Earnings Δagr

Year	n	γ_0	γ_1	R^2	$r_{\Delta agr}$	Δagr	r_{MPEG}	r_{PEG}	r_{PE}	T-Bond
1981	40	0.0189	1.0006	1.00	13.9%	0.1%	13.8%	13.8%	15.2%	14.0%
1982	50	0.0136	0.9999	1.00	11.8	0.0	11.8	11.8	11.4	10.4
1983	59	0.0135	1.0000	1.00	11.6	0.0	11.6	11.6	11.0	11.8
1984	61	0.0143	1.0481	0.99	15.6	4.8	14.5	12.8	12.3	11.6
1985	59	0.0104	1.0609	0.99	13.5	6.1	12.3	11.0	9.3	9.0
1986	61	0.0104	1.0354	0.99	13.1	3.5	11.7	10.7	8.5	7.2
1987	61	0.0140	1.0449	0.99	14.4	4.5	13.5	12.2	11.4	8.8
1988	61	0.0127	1.0416	0.98	14.3	4.2	13.1	12.0	10.7	9.1
1989	63	0.0104	1.0498	0.99	13.5	5.0	12.1	10.9	9.4	7.9
1990	61	0.0124	1.0477	0.99	14.8	4.8	13.2	11.7	10.3	8.1
1991	65	0.0117	1.0349	0.99	13.1	3.5	12.0	11.1	8.3	7.1
1992	74	0.0102	1.0344	0.99	12.8	3.4	11.2	10.8	7.4	6.7
1993	87	0.0111	1.0288	0.99	12.4	2.9	11.4	10.9	7.0	5.8
1994	98	0.0124	1.0331	0.99	13.4	3.3	12.3	11.9	8.5	7.8
1995	105	0.0101	1.0337	0.99	12.4	3.4	11.1	10.7	7.5	5.6
1996	123	0.0105	1.0302	0.99	12.3	3.0	11.0	10.8	7.1	6.4
1997	132	0.0097	1.0217	0.99	12.1	2.2	10.6	10.6	6.4	5.8
1998	130	0.0124	1.0193	0.99	12.6	1.9	11.5	11.3	7.4	4.7
1999	109	0.0116	1.0307	0.99	13.1	3.1	11.7	11.4	8.3	6.5
All	1,499	0.0117	1.0293	0.99	13.0%	2.9%	11.9%	11.3%	8.4%	

n is the number of portfolios in each year, γ_0 and γ_1 are median estimates of the coefficients from the regression of expected cum-dividend two-period-ahead earnings on expected next period's earnings deflated by current prices (that is, $ceps_{j2}/P_{j0} = \gamma_0 + \gamma_1\{eps_{j1}/P_{j0}\} + e_{j0}$ where P_{j0} is the price per share of firm j at time 0, eps_{j1} is the I/B/E/S analysts forecast of next-year earnings per share of firm j, $ceps_{j2}$ is the I/B/E/S analysts forecast of cum-dividend earnings two-years hence). Regressions are run on portfolios of 20 observations formed annually on the basis of the magnitude of the PEG ratio, R^2 is the adjusted R^2, Δagr is the median estimate of the expected change in abnormal growth in earnings estimated via Regression (15), abnormal growth in earnings is the difference between cum-dividend expected earnings in period t and earnings in period $t-1$ multiplied by the expected rate of return, $r_{\Delta agr}$ is the median estimate of the expected rate of return estimated via Regression (15). Estimates of $r_{\Delta agr}$ and Δagr are obtained by solving the following equations: $\gamma_0 = r_{\Delta agr}(r_{\Delta agr} - \Delta agr)$ and $\gamma_1 = (1 + \Delta agr)$. r_{PE} is the median estimate of the expected rate of return estimated from the PE ratio, r_{PEG} is the median estimate of the expected rate of return estimated from the PEG ratio, r_{MPEG} is the median estimate of the expected rate of return estimated using a modification to the PEG ratio that permits nonzero expected dividends; T-Bond is the ten-year U.S. government bond rate as at the date of the earnings forecast.

earnings into account is the lower estimates of the average expected equity risk premium (average of 3.1 percent compared with 4.8 percent when this change in growth is taken into account).

The column labeled r_{PE} records the median (for each year) eps_1/P_0 and may be viewed as the implied expected rate of return under the assumptions that $\Delta agr = agr_1 = 0$. The median estimate of $r_{\Delta agr}$ is higher than the median estimate of r_{PE} in all years other than 1981 and 1985. The median across all years is 8.4 percent suggesting an average bias (for this sample) of 4.6 percent downward from the estimate of $r_{\Delta agr}$ (13.0 percent).

VII. COMPARISON OF ESTIMATES OF THE EXPECTED RATE OF RETURN

Table 2 presents the Spearman rank correlations among the estimates of the portfolio expected rates of return, r_{PE}, r_{PEG}, r_{MPEG}, and $r_{\Delta agr}$, the estimate of Δagr, and the forecast of the short-term earnings growth rate (*stg*). The Spearman correlations among all of the estimates of the expected rate of return that take short-run earnings growth into account (r_{PEG}, r_{MPEG}, and $r_{\Delta agr}$) are very high. The rank correlation between r_{PEG} and r_{MPEG} is 0.99, supporting the conclusion that the implicit assumption in the PEG ratio ranking that expected dividends are zero has little effect on the ranking of stocks. The Spearman correlation between r_{PEG} and $r_{\Delta agr}$ is 0.90, suggesting that failure to take expected dividends and the change in abnormal growth in earnings into account has only a minimal effect on the ranking of stocks even though the correlation between r_{PEG} and Δagr is -0.42. The lower Spearman rank correlation between r_{PE} and $r_{\Delta agr}$ (0.48) compared with the correlation between r_{PEG} and $r_{\Delta agr}$ supports analysts' claims that the PEG ratio provides a ranking of stocks that is superior to the ranking based on the PE ratio.

Although most of the empirical analyses of the PEG ratio in the preceding sections focus on the expected rate of return (r_{PEG}) that may be estimated when the implicit assumption that $\Delta agr = 0$ is made explicit, the PEG ratio is generally used on Wall Street only as an ordinal ranking of stocks. The high correlation (0.90) between the r_{PEG} and the expected rate of return based on nonzero Δagr (that is, $r_{\Delta agr}$), suggests that the PEG ratio is a reasonable first approximation to a ranking on expected returns.

TABLE 2

Spearman Correlations among the Estimates of the Expected Rate of Return $r_{\Delta agr}$, r_{MPEG}, and r_{PEG}, r_{PE}, the Estimate of Long-Run Change in Abnormal Growth in Earnings Δagr, and the Short-Run Earnings Growth Rate (t-statistics in parentheses)

	r_{MPEG}	r_{PEG}	r_{PE}	Δagr	*stg*
$r_{\Delta agr}$	0.92 (91.09)	0.90 (87.06)	0.48 (11.51)	−0.21 (−3.81)	0.88 (12.54)
r_{MPEG}		0.99 (440.01)	0.28 (3.56)	−0.41 (−5.86)	0.93 (108.73)
r_{PEG}			0.27 (3.24)	−0.42 (−5.93)	0.95 (95.97)
r_{PE}				−0.24 (−2.99)	0.53 (12.54)
Δagr					−0.41 (−5.98)

Estimates of $r_{\Delta agr}$ and Δagr are obtained from the estimates of the intercept and slope coefficients from a regression of expected cum-dividend two-period-ahead earnings on expected next period's earnings deflated by current prices (that is, $ceps_{j2}/P_{j0} = \gamma_0 + \gamma_1\{eps_{j1}/P_{j0}\}+e_{j0}$ where P_{j0} is the price per share of firm j at time 0, eps_{j1} is the I/B/E/S analysts forecast of next-year earnings per share of firm j, $ceps_{j2}$ is the I/B/E/S analysts forecast of cum-dividend earnings two-years hence). Regressions are run on portfolios of 20 observations formed annually on the basis of the magnitude of the PEG ratio. Δagr is the estimate of the expected change in abnormal growth in earnings estimated via Regression (15), abnormal growth in earnings is the difference between cum-dividend expected earnings in period t and earnings in period $t-1$ multiplied by the expected rate of return, $r_{\Delta agr}$ is the median estimate of the expected rate of return estimated via Regression (15). Estimates of $r_{\Delta agr}$ and Δagr are obtained by solving the following equations: $\gamma_0 = r_{\Delta agr}(r_{\Delta agr} - \Delta agr)$ and $\gamma_1 = (1+\Delta agr)$. r_{PEG} is the median (across 20 observations) estimate of the expected rate of return estimated from the PEG ratio, r_{MPEG} is the median (across 20 observations) estimate of the expected rate of return estimated using a modification to the PEG ratio that permits nonzero expected dividends. Correlations are calculated each year. The mean correlations are tabled together with the associated t-statistics (that is, the mean divided by the standard error of the mean) in parentheses.

The correlations between each of the estimates of expected return that take short-run earnings growth into account ($r_{\Delta agr}$, r_{PEG}, and r_{MPEG}) and the short-term earnings growth rate (*stg*) are all high (0.88, 0.93, and 0.95, respectively). As an interesting aside, this suggests that ranking stocks on short-term growth rate may be viewed as a reasonable approximation to ranking on expected return. Note that if accounting earnings are equal to economic earnings, then the short-term earnings growth rate will be equal to the expected rate of return.

Consistent with the intuition presented via the Microsoft example in Section III, the correlation between the short-term earnings growth rate and the long-run change in abnormal growth in earnings is negative (−0.41) and significant at the 0.001 level. That is, the higher the short-term growth rate and implicitly the higher the abnormal growth in earnings "base" from which future earnings grow, the lower the change in abnormal growth in earnings beyond the two-year forecast horizon.

VIII. WHEN IS THE BIAS IN THE ESTIMATES OF THE EXPECTED RATE OF RETURN BASED ON THE PEG RATIO HIGHEST?

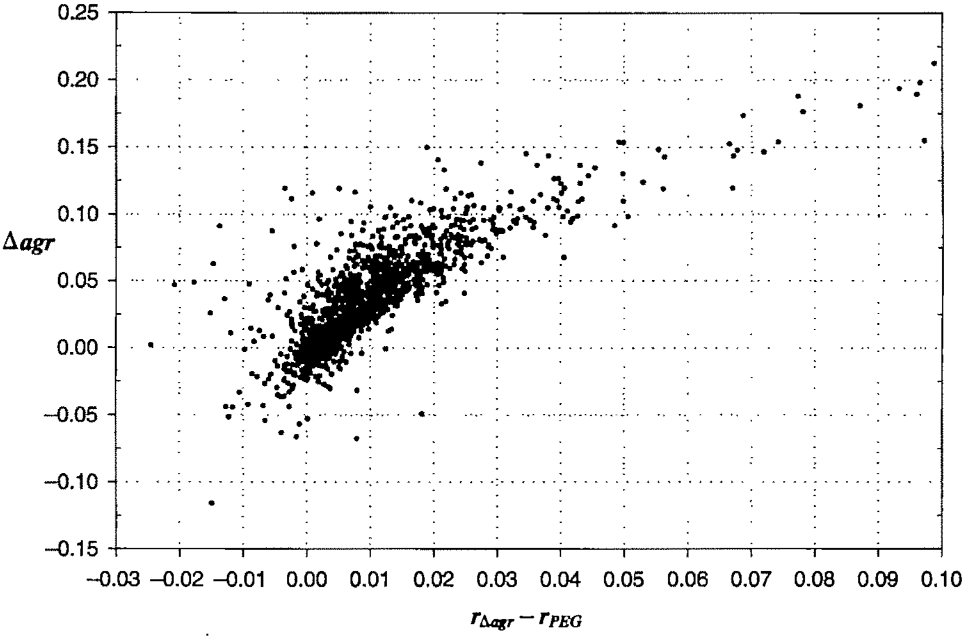
Although the correlation between the estimates of the expected rate of return based on the PEG ratio and the refined estimate of the expected rate of return is 0.90 (see Table 2), the estimates based on the PEG ratio are, on average, 1.7 percent lower than the refined estimates (see Table 1). This difference, which is considerably greater for some groups of stocks, is examined in this section.

The difference between $r_{\Delta agr}$ and r_{PEG} is the bias in the estimate of the expected rate of return when long-run change in abnormal growth in earnings (Δagr) is assumed to be zero. This difference is expected to be a function of $r_{\Delta agr}$, r_{PEG}, and the variables that affect these estimates—namely the PE ratio, expected abnormal growth in earnings (agr_1), and expected long-run change in abnormal growth in earnings (Δagr). But since these variables are all interdependent, the *ceteris paribus* assumption is unreasonable and, hence, the sign of the relation between the bias and each of these variables cannot be predicted. Nevertheless, the issue of when is the bias in the estimates of the expected rate of return based on the PEG ratio highest remains interesting as a practical empirical matter.

In this section, I examine the relation between ($r_{\Delta agr} - r_{PEG}$), $r_{\Delta agr}$, r_{PEG}, the expected short-term earnings growth rate (*stg*), and Δagr. Since cross-sectional variation in expected return (estimated by $r_{\Delta agr}$ and r_{PEG}) reflects the risk of investment in the underlying stocks, I also examine the relation between ($r_{\Delta agr} - r_{PEG}$) and several proxies for risk.

Given that the difference between $r_{\Delta agr}$ and r_{PEG} is due to the implicit assumption in the calculation of r_{PEG} that Δagr is equal to zero, I begin with an examination of the relation between ($r_{\Delta agr} - r_{PEG}$) and Δagr. Figure 1 is a scatter-plot of the 1,499 portfolio estimates of ($r_{\Delta agr} - r_{PEG}$) against Δagr. The correlation is obviously positive (Spearman correlation of 0.72 with an associated t-statistic of 9.28). The histograms of the distributions of Δagr and ($r_{\Delta agr} - r_{PEG}$) (Figures 2 and 3, respectively) provide more details of the estimates of these variables. Relatively few estimates of Δagr and ($r_{\Delta agr} - r_{PEG}$) are negative (6.08 percent and 7.04 percent, respectively). The distribution of Δagr has an overall median of 2.93 percent and the distribution of ($r_{\Delta agr} - r_{PEG}$) has an overall median of 1.37 percent. Both distributions have a long right tail. The maximum estimate of Δagr is 20.27 percent and the maximum estimate of ($r_{\Delta agr} - r_{PEG}$) is 10.76 percent. In other words, for some groups of stocks, the long-run change in abnormal growth in earnings is much greater than zero and for these stocks the estimates of the expected rate of return implied by the PEG ratio are much too low.

FIGURE 1
Scatter Plot of Long-Run Change in Abnormal Growth in Earnings on the Bias in the Estimate of Expected Rate of Return
$(r_{\Delta agr} - r_{PEG})$

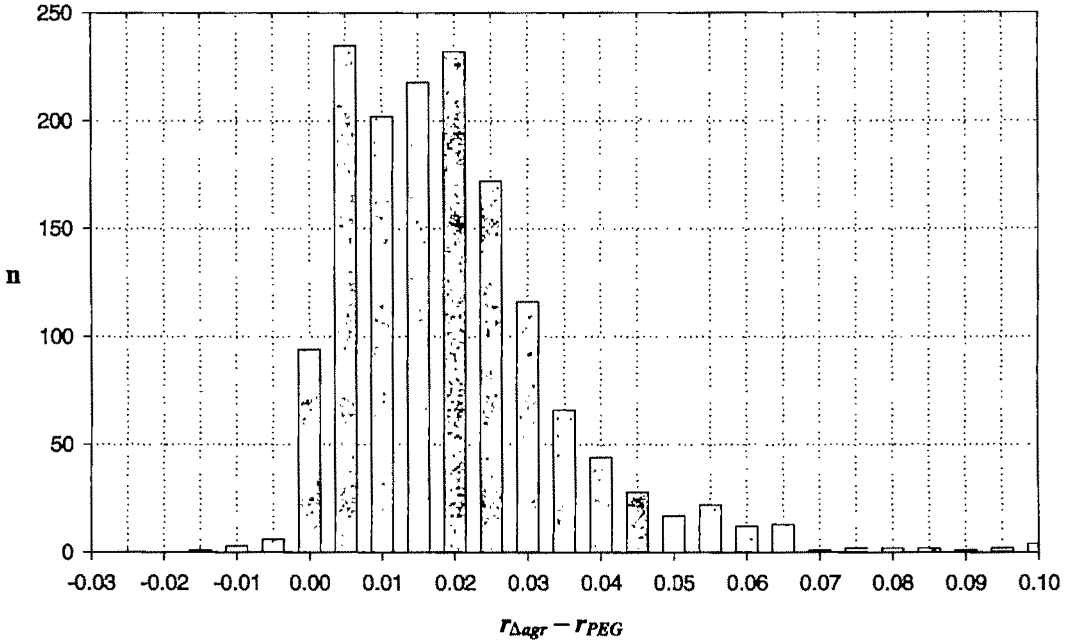


Estimates of $r_{\Delta agr}$ and Δagr are obtained from the estimates of the intercept and slope coefficients from a regression of expected cum-dividend two-period-ahead earnings on expected next period's earnings deflated by current prices (that is, $ceps_{j2}/P_{j0} = \gamma_0 + \gamma_1\{eps_{j1}/P_{j0}\} + e_{j0}$ where P_{j0} is the price per share of firm j at time 0, eps_{j1} is the I/B/E/S analysts forecast of next-year earnings per share of firm j, $ceps_{j2}$ is the I/B/E/S analysts forecast of cum-dividend earnings two-years hence). Regressions are run on portfolios of 20 observations formed annually on the basis of the magnitude of the PEG ratio. Δagr is the median estimate of the expected change in abnormal growth in earnings estimated via Regression (15), abnormal growth in earnings is the difference between cum-dividend expected earnings in period t and earnings in period $t-1$ multiplied by the expected rate of return, $r_{\Delta agr}$ is the median estimate of the expected rate of return estimated via Regression (15). Estimates of $r_{\Delta agr}$ and Δagr are obtained by solving the following equations: $\gamma_0 = r_{\Delta agr}(r_{\Delta agr} - \Delta agr)$ and $\gamma_1 = (1+\Delta agr)$. r_{PEG} is the estimate of the expected rate of return estimated from the PEG ratio.

The question remains as to whether we can *a priori* identify stocks for which we would expect the bias in the estimate of the expected rate of return based on the PEG ratio (that is, $r_{\Delta agr} - r_{PEG}$) to be greater. With this in mind, I examine the relations between the bias $(r_{\Delta agr} - r_{PEG})$, various risk proxies, and short-term earnings growth forecasts (*stg*). The Spearman rank correlations between $(r_{\Delta agr} - r_{PEG})$ and several proxies for risk are included in Table 3. The correlations between short-term earnings growth rate (*stg*) and the risk proxies are also presented as they may help in the interpretation of the former set of correlations. Before discussing the correlations, I provide a brief justification for the choice of risk proxies.

Since Sharpe (1964), Lintner (1965), and Mossin (1966) beta has been used as an indication of systematic risk. Beta is estimated for each firm using the 60-month return period prior to the earnings forecast date. Malkiel (1997) focuses on total risk as measured

FIGURE 2
Histogram of the Distribution of the Bias in the Estimate of Expected Rate of Return
$(r_{\Delta agr} - r_{PEG})$

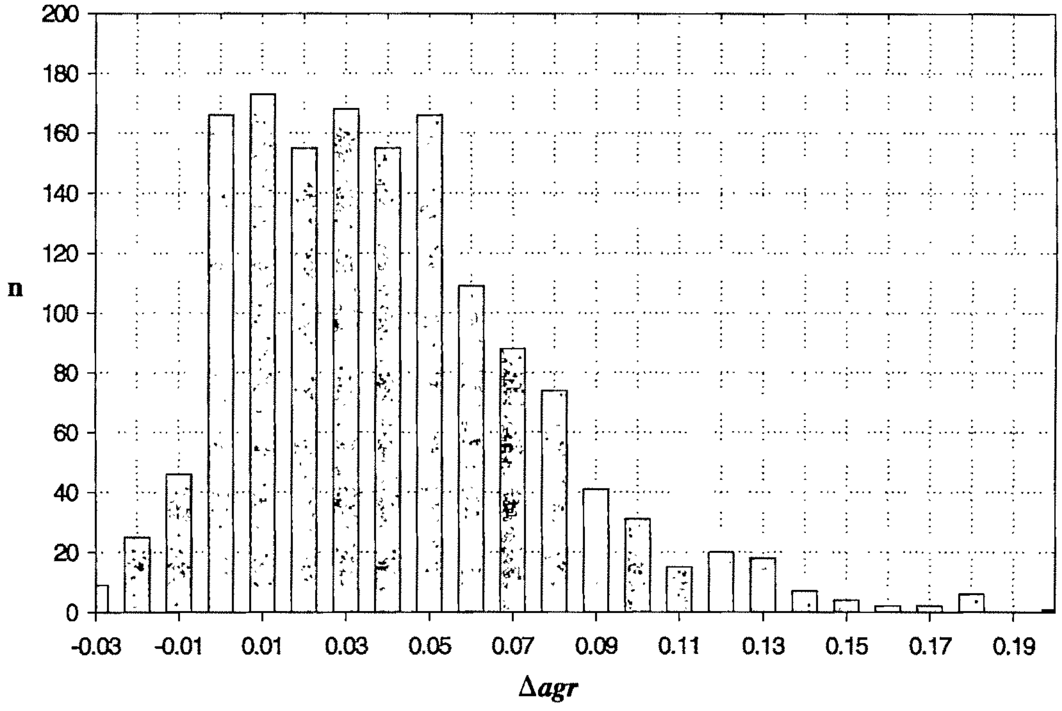


n is number of observations. Estimates of $r_{\Delta agr}$ and Δagr are obtained from the estimates of the intercept and slope coefficients from a regression of expected cum-dividend two-period-ahead earnings on expected next period's earnings deflated by current prices (that is, $ceps_{j2}/P_{j0} = \gamma_0 + \gamma_1\{eps_{j1}/P_{j0}\} + e_{j0}$ where P_{j0} is the price per share of firm j at time 0, eps_{j1} is the I/B/E/S analysts forecast of next-year earnings per share of firm j, $ceps_{j2}$ is the I/B/E/S analysts forecast of cum-dividend earnings two-years hence). Regressions are run on portfolios of 20 observations formed annually on the basis of the magnitude of the PEG ratio. Estimates of $r_{\Delta agr}$ and Δagr are obtained by solving the following equations: $\gamma_0 = r_{\Delta agr}(r_{\Delta agr} - \Delta agr)$ and $\gamma_1 = (1 + \Delta agr)$. r_{PEG} is the estimate of the expected rate of return estimated from the PEG ratio.

by the variance of returns. I use the standard deviation of daily returns prior to the forecast date as a measure of total risk. Modigliani and Miller (1958) predict that the expected rate of return on equity capital will increase with the amount of debt in the firm's capital structure and Fama and French (1992) provide evidence based on *ex post* realized returns that is consistent with this prediction. I use the ratio of total long-term debt to the market value of equity at the earnings forecast date as the measure of the amount of debt in the firm's capital structure. Numerous studies, for example, Banz (1981) and Fama and French (1992), have documented a negative association between market capitalization and realized returns suggesting the market capitalization (which I measure at the earnings forecast date) may be used as a proxy for risk. Finally, consistent with Fama and French (1992) and Berk et al. (1999), I use the ratio of the book value of common equity to the market value of common equity at the earnings forecast date as another measure of risk.

Although the correlation between the short-term earnings growth rate (*stg*) and beta, the debt-to-market ratio, and the book-to-market ratio are not significantly different from zero at the 0.05 level, the correlations between the short-term earnings growth rate and two

FIGURE 3
Histogram of the Distribution of the Estimate of Long-Run Change in Abnormal Growth in Earnings
Δagr



n is number of observations. Estimates of Δagr are obtained from the estimates of the intercept and slope coefficients from a regression of expected cum-dividend two-period-ahead earnings on expected next period's earnings deflated by current prices (that is, $ceps_{j2}/P_{j0} = \gamma_0 + \gamma_1\{eps_{j1}/P_{j0}\}+e_{j0}$ where P_{j0} is the price per share of firm j at time 0, eps_{j1} is the I/B/E/S analysts forecast of next-year earnings per share of firm j, $ceps_{j2}$ is the I/B/E/S analysts forecast of cum-dividend earnings two-years hence). Regressions are run on portfolios of 20 observations formed annually on the basis of the magnitude of the PEG ratio. Estimates of Δagr are obtained by solving the following equations: $\gamma_0 = r_{\Delta agr}(r_{\Delta agr} - \Delta agr)$ and $\gamma_1 = (1 + \Delta agr)$.

of the risk proxies (standard deviation of returns and market capitalization) are high and in the predicted direction (0.70 and −0.54, respectively). This, and the very high correlation between the short-term earnings growth rate (*stg*) and expected return (0.88—see Table 2), suggests that short-term earnings growth rate (*stg*) may be viewed as a reasonable proxy for risk.[35]

Proponents of the PEG ratio as a means of ranking stocks argue that it is most effective when short-term growth is high and, consistent with this argument, the correlation between the short-term earnings growth rate and the bias ($r_{\Delta agr} - r_{PEG}$) in the estimate of expected returns is negative (−0.52) and significant at the 0.001 level. Consistent with this high

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[35] The correlations between $r_{\Delta agr}$, r_{PEG}, and r_{MPEG} and each of the risk proxies (not reported) are very similar to the correlation between *stg* and each of these proxies.

TABLE 3

Spearman Correlations between the Bias in the Estimate of the Expected Rate of Return Based on the Peg Ratio ($r_{\Delta agr} - r_{peg}$), the Estimates of the Expected Rate of Return $r_{\Delta agr}$, r_{peg}, the Expected Short-Term Earnings Growth Rate, the Long-Run Change in Abnormal Growth in Earnings Δagr, and Several Proxies for Risk

	β	SD_{ret}	*size*	*leverage*	*BP*	*EP*
stg						
corr	−0.02	0.70	−0.54	0.02	0.08	0.53
t-stat	(−0.64)	(22.40)	(−8.90)	(0.31)	(1.17)	(12.54)
$r_{\Delta agr} - r_{PEG}$						
corr	0.01	−0.48	0.40	−0.03	−0.10	−0.42
t-stat	(0.53)	(−8.39)	(6.57)	(−0.94)	(−2.08)	−6.67
	stg	$r_{\Delta agr}$	r_{MPEG}	r_{PEG}	Δagr	
$r_{\Delta agr} - r_{PEG}$						
corr	−0.52	−0.32	−0.51	−0.54	0.72	
t-stat	(−7.27)	(−5.64)	(−7.03)	(−7.21)	9.28	

stg is the expected short-term earnings growth rate. Estimates of $r_{\Delta agr}$ and Δagr are obtained from the estimates of the intercept and slope coefficients from a regression of expected cum-dividend two-period-ahead earnings on expected next period's earnings deflated by current prices (that is, $ceps_{j2}/P_{j0} = \gamma_0 + \gamma_1\{eps_{j1}/P_{j0}\} + e_{j0}$ where P_{j0} is the price per share of firm j at time 0, eps_{j1} is the I/B/E/S analysts forecast of next-year earnings per share of firm j, $ceps_{j2}$ is the I/B/E/S analysts forecast of cum-dividend earnings two-years hence). Regressions are run on portfolios of 20 observations formed annually on the basis of the magnitude of the PEG ratio. Δagr is the median estimate of the expected change in abnormal growth in earnings estimated via Regression (15), abnormal growth in earnings is the difference between cum-dividend expected earnings in period t and earnings in period $t-1$ multiplied by the expected rate of return, $r_{\Delta agr}$ is the median estimate of the expected rate of return estimated via Regression (15). Estimates of $r_{\Delta agr}$ and Δagr are obtained by solving the following equations: $\gamma_0 = r_{\Delta agr}(r_{\Delta agr} - \Delta agr)$ and $\gamma_1 = (1 + \Delta agr)$. r_{PEG} is the estimate of the expected rate of return estimated from the PEG ratio, r_{MPEG} is the estimate of the expected rate of return estimated using a modification to the PEG ratio that permits nonzero expected dividends. β is the capital asset pricing model beta estimated over the 60 months prior to fiscal year end, SD_{ret} is the standard deviation of the previous year's daily returns; *leverage* is the ratio of book value of long-term debt to the market value of equity, *size* is the market capitalization, *BP* is the ratio of the book value of common equity to the market value of common equity, *EP* is the ratio of the next period expected earnings to price per share. All variables other than $r_{\Delta agr}$ and Δagr are calculated at the firm-specific level and the median across the 20 observations is used in the calculation of the correlations. Where an observed variable is missing, the median of the remaining observations is used. Correlations are calculated each year. The mean correlations are tabled together with the associated t-statistics (that is, the mean divided by the standard error of the mean) in parentheses.

correlation and the correlation between the short-term earnings growth rate and risk, the bias is also correlated with the risk measures with which the short-term earnings growth rate is correlated (the correlation between [$r_{\Delta agr} - r_{PEG}$] and the standard deviation of returns is −0.48 and the correlation between [$r_{\Delta agr} - r_{PEG}$] and the market capitalization is 0.40). The correlation between ($r_{\Delta agr} - r_{PEG}$) and the book-to-market ratio is −0.10 and significant at the 0.05 level, while the correlation between ($r_{\Delta agr} - r_{PEG}$) and the earnings-to-price ratio is −0.42 and significant at the 0.01 level. Consistent with these indications of a negative relation between risk and the bias in the estimate of the expected rate of return, the correlation between ($r_{\Delta agr} - r_{PEG}$) and $r_{\Delta agr}$ is negative (−0.32) and significant at the 0.001 level.

TABLE 4
Bias in the Estimate of the Expected Rate of Return Based on the PEG Ratio ($r_{\Delta agr} - r_{PEG}$), and Various Firm Characteristics

	Portfolio			
Basis of Grouping	1 (lowest)	2	3	4 (highest)
PE	0.78%	1.26%	1.86%	2.09%
PEG	0.51	1.08	1.77	2.55
β	1.34	1.59	1.51	1.45
SD_{ret}	2.29	1.76	1.25	0.68
size	0.68	1.25	1.89	2.09
leverage	1.64	1.56	1.38	1.25
BP	1.69	1.49	1.49	1.07
stg	2.50	1.76	1.16	0.54

Reported percentages are median differences between $r_{\Delta agr}$ and r_{PEG} for quartiles of observations formed annually based on the magnitude of the firm characteristics. Estimates of $r_{\Delta agr}$ are obtained from the estimates of the intercept and slope coefficients from a regression of expected cum-dividend two-period-ahead earnings on expected next period's earnings deflated by current prices (that is, $ceps_{j2}/P_{j0} = \gamma_0 + \gamma_1\{eps_{j1}/P_{j0}\} + e_{j0}$ where P_{j0} is the price per share of firm *j* at time 0, eps_{j1} is the I/B/E/S analysts forecast of next-year earnings per share of firm *j*, $ceps_{j2}$ is the I/B/E/S analysts forecast of cum-dividend earnings two-years hence). Regressions are run on portfolios of 20 observations formed annually on the basis of the magnitude of the PEG ratio. Estimates of $r_{\Delta agr}$ are obtained by solving the following equations: $\gamma_0 = r_{\Delta agr}(r_{\Delta agr} - \Delta agr)$ and $\gamma_1 = (1+\Delta agr)$. PE is the median ratio of price-to-next-period earnings for the portfolio of 20 stocks, PEG is the median PEG ratio for the portfolio of 20 stocks. β is capital asset pricing model beta estimated over the 60 months prior to fiscal year end, SD_{ret} is the standard deviation of the previous year's daily returns, *leverage* is the ratio of book value of long-term debt to the market value of equity, *size* is the market capitalization (in millions of dollars), *BP* is the ratio of the book value of common equity to the market value of common equity, *stg* is the expected short-term earnings growth rate. All variables other than $r_{\Delta agr}$ and Δagr are calculated at the firm-specific level and the median across the 20 observations is used in the calculation of the correlations. Where an observed variable is missing, the median of the remaining observations is used. Correlations are calculated each year. The mean correlations are tabled together with the associated t-statistic (that is, the mean divided by the standard error of the mean) in parentheses.

To summarize, these correlations suggest that the bias in the estimate of the expected rate of return based on the PEG ratio will be higher for firms with higher PE, higher PEG ratios, lower book-to-price ratios, lower standard deviation of past returns, and higher market capitalization. To gain an indication of the relative magnitudes of these biases, firms were formed each year into four portfolios based on the ranking of each of the variables. The median biases for each of these portfolios are presented in Table 4. Consistent with the correlations in Table 3, the bias is considerably higher for high PE and high PEG firms (2.09 percent compared with 0.78 percent and 2.55 percent compared with 0.51 percent). Similarly, it is higher (2.29 percent compared with 0.68 percent) when the standard deviation of returns is lower and it is higher for large firms (2.09 percent compared with 0.68 percent). Although the difference is less across the book-to-price ratio portfolios, the difference is consistent with the negative correlation (lower bias for higher book-to-price ratio stocks—1.69 percent compared with 1.07 percent). The bias is also considerably higher for firms with lower expected short-term earnings growth rate (2.50 percent compared with 0.54 percent).

IX. SUMMARY AND CONCLUSIONS

I describe a model of earnings and earnings growth and I demonstrate how the model may be used to obtain estimates of the cost of equity capital. Just as in the residual income model where future residual income is nonzero if price is not equal to book value (that is, future residual income represents the future earnings adjustment—growth in book value—to recognize the difference between price and book value) future abnormal growth in earnings adjusts for the difference between next period's accounting earnings and next period's economic earnings. Analysts' reports still pervasively focus on forecasts of earnings and earnings growth rather book value and the forecasts of book value growth that are implicit in the residual income valuation model. In other words, analysts' reports have an earnings focus rather than a book value (or balance sheet) focus. The model and the empirical analyses in this paper also focus on earnings.

I develop and demonstrate a procedure for simultaneously estimating the implied market expectation of the rate of return and the implied market expectation of the long-run change in abnormal growth in earnings (beyond a short earnings forecast horizon) for a portfolio of stocks. The method is applied to portfolios of stocks formed according to the magnitude of the PEG ratio. Inputs to the estimation procedure are current prices and forecasts of earnings and of the short-term earnings growth rate. Estimates of the expected rate of return obtained via this procedure are compared with the estimates implicit in the PEG ratio and PE ratio heuristics.

Market prices imply that the market expects abnormal growth in earnings to change at an average rate of 2.9 percent per year beyond the two-year forecast horizon. It follows that the estimates of the expected rate of return based on the PEG ratio are, on average, 1.7 percent lower than the estimates that take this growth into account. Thus, although the high rank correlation between the two estimates of the expected rate of return may suggest that the PEG ratio is a useful parsimonious means of ranking stocks, the bias is considerable. Estimates of the bias are very high for some groups of stocks. Nevertheless, consistent with the arguments of the proponents of the PEG ratio, the bias is much less than the bias in estimates of the expected rate of return based on the PE ratio.

The general downward bias in the estimate of the expected rate of return based on the PEG ratio is shown to be higher for firms with higher PE, higher PEG ratios, lower book-to-price ratios, lower standard deviation of past returns, higher market capitalization, and lower expected short-term earnings growth rates.

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Unrecognized Deferred Taxes: Evidence from the U.K.

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ABSTRACT: We examine whether U.K. managers use the flexibility provided under the partial method for deferred taxes to measure unrecognized deferred taxes opportunistically. We first test whether firm-specific operational and opportunistic factors are associated with the level of unrecognized deferred taxes. The tests provide evidence certain U.K. managers opportunistically measure deferred taxes to manage leverage, consistent with arguments by commentators that deferred taxes heavily influence leverage indicators that play a prominent role in the U.K. contracting framework. Because the proper identification and measurement of both operational and opportunistic determinants of unrecognized deferred taxes influence our tests, we additionally investigate whether unrecognized taxes relate to future deferred tax reversals and future operating profitability of the firm. These tests show the components of deferred taxes predict both future deferred tax reversals and indicators of future profitability of the firm as predicted. Taken together, our results indicate that, on average, the existence of balance sheet management does not nullify the predictive power of (unrecognized) deferred taxes for future deferred tax reversals and for profitability measures. One implication of the results is that the recent U.K. standard change eliminating the partial provision method for deferred taxes potentially has reduced the usefulness of deferred tax disclosures.

Keywords: *deferred taxes; earnings management; accounting standard; international accounting.*

Data Availability: *Data are available from public sources as indicated in the text.*

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I. INTRODUCTION

We test whether managers use the flexibility provided under the partial method of deferred taxation accounting in the U.K. to manage earnings opportunistically. The relative merits of the partial and the full method of accounting for deferred taxes have long been the subject of debate among academics, practitioners, and regulators. Until recently, U.K. GAAP prescribed the partial method, allowing U.K. managers to record deferred tax assets and liabilities only for timing differences that will reverse in the foreseeable future without being replaced (Statement of Standard Accounting Practice [SSAP] No. 15, Accounting Standards Committee [ASC] 1999, para. 12). In contrast, under U.S. GAAP, managers must record the full amount of timing differences regardless of when they will reverse. Using the partial method, U.K. managers therefore have the ability and discretion to communicate private information about expected operational activity, such as future expected cash flows and growth, to the market. Conversely, managers could exercise discretion opportunistically in estimating the unrecognized amounts to manage the balance sheet and/or the income statement. Using the disclosure of unrecognized deferred taxes in the notes to U.K. annual reports, we investigate whether U.K. managers use their discretion under the partial method to determine unrecognized deferred taxes opportunistically.

Our study contributes to and extends U.S.-based research on the use of deferred taxes to manage earnings. The U.S.-based research, focusing on management of the valuation allowance of deferred tax assets, fails to provide conclusive evidence of management opportunism related to deferred taxes. Schrand and Wong (2002) find evidence of income smoothing using the deferred tax asset valuation allowance; however, their study is limited to the banking industry. In a more general cross-sectional setting, Miller and Skinner (1998) find no evidence of management opportunism. They conclude the most important explanatory variable for the valuation allowance of deferred tax assets is the level of firms' tax credit and tax loss carryforwards, consistent with those items being more difficult to realize (Miller and Skinner 1998, 23). The generalizability of the U.S. results therefore appears to be limited to a specific timing difference or industry.

U.S. research on deferred taxes and management opportunism also faces the constraint that U.S. GAAP permits a valuation allowance only for deferred tax assets. In contrast, the partial method under U.K. GAAP allows U.K. managers to record "valuation allowances" for deferred tax assets *and* liabilities leading to both deferred tax assets and liabilities off the balance sheet.[1] Our descriptive statistics show seven times as many companies report unrecognized deferred tax liabilities (i.e., the U.S. equivalent of a valuation allowance for deferred tax liabilities) than those reporting unrecognized deferred tax assets, implying our results generalize to a broad group of companies. We also find the median absolute value of unrecognized deferred tax liabilities (scaled by market value) to be twice as large as the median absolute value of unrecognized deferred tax assets, suggesting managers have a lot of flexibility to report unrecognized deferred tax liabilities. Additionally, and more importantly, the U.K. institutional context emphasizes the role of balance sheet leverage ratios as financial covenants (see Day and Taylor 1996). Consequently, since managers can influence leverage ratios through the measurement of deferred taxes, this accounting item is a potentially important opportunistic reporting tool in the U.K. setting.[2]

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[1] We use the terms "recognized" and "unrecognized deferred taxes" to distinguish between the two components of deferred taxes under the partial method. The unrecognized portions of deferred taxes are similar to the valuation allowances reported in the U.S.

[2] Leverage ratios often relate measures of debt or liabilities to measures of shareholders' equity. Managers can affect leverage ratios through their choice between recognized and unrecognized deferred taxes, thus influencing both total liabilities and shareholders' equity (through net income).

We carry out two different tests to address our main research question. First, we test whether firm-specific operational and opportunistic factors are associated with the level of unrecognized deferred taxes. In particular, we examine both operational (e.g., capital intensity) and opportunistic determinants of deferred taxes, considering two potential opportunistic strategies focusing on either leverage or earnings smoothing (see Miller and Skinner 1998). Our results indicate operational factors relate significantly to levels of and changes in *both* unrecognized deferred tax assets and liabilities, implying management estimates deferred taxes based on underlying economic factors.[3] However, we also find our proxy for opportunistic management behavior to influence leverage is associated with the level of unrecognized deferred taxes for a subset of firms. The results of our first tests suggest U.K. managers, to a certain extent, measure unrecognized deferred taxes opportunistically to influence leverage but not to smooth earnings. The finding is consistent with arguments by commentators that deferred taxes influence indicators of leverage that play a prominent role in the U.K. contracting framework (Citron 1992; Day and Taylor 1996).

The validity of the first set of tests relies on our correct identification and measurement of the operational and opportunistic determinants of recognized and unrecognized deferred taxes. However, we also estimate a second set of prediction tests not influenced by our choice of determinants. First, we examine whether recognized and unrecognized deferred tax amounts relate to future reversal of deferred taxes. We base our reversal test specification on the fundamental characteristic of the partial method that U.K. managers *only* have to record deferred tax assets and liabilities for timing differences that will reverse in the foreseeable future. If U.K. managers apply the partial method appropriately (i.e., not opportunistically), recognized and unrecognized deferred taxes will predict reversals of timing differences. We find both recognized and unrecognized deferred taxes reliably predict future deferred tax reversals over a three-year period, consistent with the specification in the accounting standard.

In the second prediction test, we examine the relation between the components of deferred taxes and future profitability. Since management can signal its private information about future operating profitability with the components of deferred taxes, we predict that unrecognized deferred tax liabilities (assets) will be positively (negatively) related to future profitability. Focusing on different proxies for future profitability such as cash flow from operations, *EBITDA*, and operating income, we find strong evidence that unrecognized deferred taxes relate to proxies for the future profitability of the firm, consistent with management using unrecognized deferred taxes to signal future profitability of the firm.

To summarize, our results are consistent with certain U.K. managers measuring deferred taxes opportunistically to manage the balance sheet but not the income statement. Our prediction tests, however, indicate that, on average, the existence of balance sheet management does not nullify the predictive power of (unrecognized) deferred taxes for future deferred tax reversals and for profitability measures. Our study thus provides evidence pertaining to the debate among academics and regulators on the relative merits of the partial provision method. The debate has focused predominantly on the valuation-relevance of deferred taxes as a function of the timing of the reversals of deferred tax accounts. Although the Accounting Standards Board (ASB), the U.K. regulator, seems to believe timing differences determine the valuation-relevance of deferred taxes, it recently eliminated the partial provision method primarily to make U.K. GAAP for deferred taxes more consistent with international practice. Specifically, relative to U.S. GAAP, the change in standard eliminates U.K. managers' opportunity to influence the measurement of deferred tax liabilities on the balance sheet.[4] Our results suggest though that allowing managers to report when timing differences are expected to reverse through their estimates of unrecognized deferred taxes conveys information about future profitability of the firm. We conclude therefore that eliminating the partial provision method for deferred taxes reduces the usefulness of deferred tax disclosures.[5] It is a case where accounting harmonization for the sake of harmonization does not necessarily result in more useful financial statements.

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[3] In our tests, we examine aggregate net deferred tax assets/liabilities because the database we use to collect financial statement information, Datastream International, does not provide the detailed components of recognized deferred taxes and unrecognized deferred taxes.

The remainder of this paper is organized as follows. Section II describes U.K. GAAP for deferred taxation; Section III motivates our analyses and discusses related research. Section IV describes U.K. deferred taxation activity. Sections V and VI discuss and present findings from our tests. Section VII concludes.

II. U.K. ACCOUNTING FOR DEFERRED TAX

Until December 2000, the U.K.'s authoritative standard on accounting for deferred income taxes was SSAP No. 15, *Accounting for Deferred Tax,* which prescribed the partial provision method of accounting for deferred taxation. Under the U.K.'s partial method of accounting for deferred taxes, deferred tax liabilities and assets should *only* be recognized (provided) for timing differences that will reverse in the foreseeable future without being replaced (SSAP No. 15, para. 12).[6] Under SSAP No. 15, management assesses whether deferred tax liabilities or assets will reverse in the foreseeable future, generally considered to be within the next three to five years (SSAP No. 15, Appendix). The standard recognizes that factors such as growth, estimates of asset useful lives, asset impairments, and plans for plant closings will influence management's assessment. SSAP No. 15 allows management to consider the *combined* effect of all timing differences rather than looking at individual categories in isolation when determining deferred tax to be recognized. Management computes deferred tax under the liability method and discloses recognized and unrecognized amounts of deferred tax relating to the current period income. Management also discloses major components of recognized and unrecognized deferred tax liabilities/assets. Common timing differences relate to, among others, advance corporation tax (ACT), depreciation (U.K. terminology: accelerated capital allowances), previous operating losses, and revaluations.

Our study is timely since the U.K. ASB recently issued Financial Reporting Standard (FRS) No. 19, *Deferred Tax,* discontinuing the use of the partial method in favor of the full method. The debate in the U.K. on the partial versus the full provision method has been particularly passionate. Proponents of the partial method argue the full provision method leads to recognition of large sums of deferred taxes that have only a remote possibility of becoming payable in the future. Their main concern is that if companies are required to recognize these tax liabilities, then their leverage ratios potentially give the impression of poor creditworthiness. Additionally, proponents of the partial method argue that, if borrowing capacity is computed with respect to shareholders' equity, firms could be adversely affected because the full method of deferred taxes understates retained earnings. In other words, proponents argue the partial method leads to the recognition of a realistic level of liabilities (Davies et al. 1997, 1199).

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[4] The new standard still is noticeably different from international practice in certain respects. For example, it permits discounting of deferred tax assets and liabilities to reflect the time value of money (para. 42). In addition, the ASB believes that deferred taxes should be recorded only when they satisfy the criteria for liabilities or assets. In other words, they represent obligations to pay more or rights to pay less tax in the future. As a result, under the new standard, companies need only recognize deferred taxes to the extent that there is a *taxable event.* For example, when a company revalues an asset, it has to record deferred taxes only if it commits to sell the asset. The ASB's view on deferred tax assets and liabilities is referred to as the incremental liability approach and contrasts with the valuation adjustment approach adopted for example by Sansing (1998) and Guenther and Sansing (2000).

[5] Still, accounting for deferred taxation remains one of the major accounting differences impeding comparison among countries as many countries continue to use some form of the partial method of deferred taxation. Some countries allowing some form of the partial provision method of accounting for deferred taxation include France (allows comprehensive and partial in consolidated statement only), Hong Kong, Italy, Mexico, New Zealand (allows comprehensive and partial), and Singapore (Afterman 2000). For a comprehensive analysis of differences in accounting across countries, see Harris (1998).

[6] Statement of Standard Accounting Practice (SSAP) No. 15, *Accounting for Deferred Tax,* was the U.K.'s authoritative standard on accounting for deferred taxation from 1978.

In contrast, opponents of the partial method argue the method is subjective and inconsistent with principles underlying other aspects of accounting (FRS No. 19, Appendix V, para. 14). In particular, when using the partial method, management potentially wrongly forecasts future events (e.g., a certain level of future capital expenditure). Also, rather than accounting for transactions individually, the partial method accounts for effects of current transactions in combination with effects of transactions that have not yet occurred (Davies et al. 1997, 1199).[7] Opponents also point out variation exists in the application of the partial provision method. FRS No. 19 states explicitly that "different entities within the same industry and with similar prospects seem to take quite different views on the levels of provision that are necessary" (FRS No. 19, Appendix V, para 14).

III. MOTIVATION AND RELATION TO PRIOR RESEARCH

Using the disclosure of unrecognized deferred taxes in the notes to U.K. annual reports, we investigate whether U.K. managers use their discretion under the partial method to communicate private information to the market about expected operational activity or to opportunistically manage the balance sheet and/or the income statement. Our study extends previous research on deferred taxes and earnings management in the U.S. A number of studies focus on the link between earnings management and deferred taxes, in particular the valuation allowance of deferred tax assets. Behn et al. (1998) and Miller and Skinner (1998) study the determinants of the valuation allowance for deferred tax assets and conclude that, in general, management implements SFAS No. 109 as intended, and does not manipulate earnings through the valuation allowance of deferred tax assets. However, the authors acknowledge their evidence is not conclusive since their tests lack power (Miller and Skinner 1998, 232). More recently, Schrand and Wong (2002) focus on a specific industry, the banking sector, with large deferred tax assets to re-examine the question. In contrast with Miller and Skinner (1998), Schrand and Wong (2002) find evidence of earnings smoothing through the valuation allowance for deferred tax assets. They ascribe the inconsistency between their results and those of Miller and Skinner (1998) to the difference in setting: the banking sector provides a more powerful setting to test the use of the valuation allowance for earnings management purposes. By the same token, Schrand and Wong (2002) acknowledge the reduced generalizability of their results.

A focus on the U.K. allows us to design a powerful test of the hypothesized link between opportunistic financial reporting and the valuation allowance for deferred taxes in a general cross-sectional setting for two reasons. First, the U.K. GAAP partial method standard allows managers to report unrecognized deferred tax assets *and* liabilities. As previously mentioned, both the number of firms reporting an unrecognized deferred tax liability and the amount of the unrecognized deferred tax liability significantly exceed those relating to unrecognized deferred tax assets. Thus, U.K. managers have a potentially effective tool for opportunistic reporting purposes not available in the U.S. Second, and more importantly, U.K. managers potentially face strong incentives to influence leverage indicators through their measurement of unrecognized deferred taxes. Commentators argue indicators of leverage are more prominent in the U.K. contracting framework than in the U.S. Because leverage ratios often relate measures of debt or liabilities to measures of shareholders' equity, managers can influence them through their estimation of deferred taxes since the choice between recognized and unrecognized deferred taxes affects both total liabilities and shareholders' equity (through net income). We mention earlier how proponents of the partial method refer to the link between deferred tax measurement and leverage to advocate the method. Day and Taylor (1998) discuss how the role of debt contracts is different in the U.K. versus the U.S. They show empirical evidence from Day and Taylor (1996) and Citron (1992) on the important role of three financial covenants in the design of debt contracts: interest coverage, balance sheet gearing, and minimum tangible net worth. Similarly, Day and Taylor (1997) find that, in contrast to the U.S. practice, cash flow or dividend-based covenants are rarely used.

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[7] To illustrate these problems, the partial provision method was not regarded as appropriate for dealing with the long-term deferred tax assets associated with the provisions for post-retirement benefits. In implementing the standard, it is the one exception where management can consider the timing effects of the individual item rather than combining the item with all other timing differences. It was argued that the deferred tax asset was as real an asset as the post-retirement liability a liability.

Overall, the U.K. focus on balance sheet measures reinforces the importance of unrecognized deferred tax calculations. Citron (1992) finds most covenant calculations rely on "rolling" U.K. GAAP numbers, meaning that the covenants are based on GAAP in use at the date of computation. As a result, new accounting standards can cause covenant breaches (Citron 1992, 25). We find anecdotal evidence that the concern over covenant breaches existed at the time the ASB changed the U.K. standard for deferred taxes in the quarterly accounting briefing by PricewaterhouseCoopers of December 2000. The briefing states explicitly that the move from the partial to the full provision method of deferred taxes will increase gearing, reduce distributable profits, and reduce net assets. The briefing continues to point out that, as a consequence, the accounting change might impact the banking covenants of firms. The concern highlights the potential importance of the unrecognized deferred taxes in the context of debt contracts in the U.K.[8]

As further indication of the concern over the effect of the new standard on the balance sheet, PricewaterhouseCoopers reports in a later quarterly accounting briefing of August 2001 that 40 large U.K. companies surveyed generally elect not to adopt the new deferred tax standard (FRS No. 19) early in part because of its perceived adverse effect on the balance sheet. Although both unrecognized deferred tax assets and liabilities affect measures of leverage, the anecdotal evidence suggests practitioners are predominantly concerned by the effects of the new standard on the magnitude of liabilities on the balance sheet and the computation of leverage ratios. Even if the adoption of the standard will not always lead to violations of debt covenants, practitioners highlight (by referring to calculations by PricewaterhouseCoopers) that the introduction of the new standard will add an average of 12 percent points to the leverage of U.K. firms (*Investors Chronicle* 2002). As a practical example, British Petroleum's earnings press release after adoption of FRS No. 19 indicates the firm raises its leverage band to offset the impact of FRS No. 19 (British Petroleum

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[8] We find similar arguments in a comment letter on the proposal for the new standard in the U.K., written on December 6, 1999, by the Association of Corporate Treasurers in the U.K.

2002). Our discussion, therefore, suggests the specific objectives of U.K. deferred tax management are balance sheet rather than income statement oriented, in contrast to the U.S. setting (e.g., notice the focus on earnings smoothing in Schrand and Wong (2002)).[9]

IV. DESCRIPTION OF U.K. SAMPLE AND DEFERRED TAXATION ACTIVITY

We obtain our data from Datastream International, which includes market and accounting data for all U.K. industrial commercial firms. Like Aboody et al. (1999), we use the comprehensive U.K.-domiciled Datastream International database to collect information on variables we hypothesize to be related to deferred tax activity of U.K. firms. Datastream International reports net recognized and net unrecognized deferred tax assets/liabilities as well as the current period deferred tax expense.[10] Unfortunately, Datastream International does not provide information that allows us to identify the detailed components of recognized deferred taxes (*RDT*) and unrecognized deferred taxes (*UDT*).

After deleting observations with missing variables (2,928), changes in year-ends (142), and outliers (311), our sample comprises 3,912 observations associated with 809 nonfinancial firms from fiscal years 1993 through 1998 as shown in Table 1, Panel A.[11] We exclude financial firms from our sample because major components of deferred taxes such as depreciation are associated with nonfinancial firms and the characteristics of financial firms likely differ from nonfinancial firms. Our sample is composed of firms in 34 different industries (as defined by the database). Unreported analyses show a wide range of industries contain observations with either assets, liabilities, or zero unrecognized deferred taxes; that is, we find no concentration of a particular type of unrecognized deferred taxes in one industry. The pattern suggests that differences in industrial composition across groups do not drive our results.

Table 1, Panel B, presents frequencies and magnitudes of *RDT* and *UDT*, both divided by market values, in our sample. We present results partitioned on the nature of the unrecognized deferred taxes for two reasons: (1) we focus on the determinants of unrecognized deferred taxes; and (2) the determinants of deferred tax assets are likely to be different from those of deferred tax liabilities. Consistent with the Datastream International database, we code *net* deferred tax liabilities as positive variables and *net* deferred tax assets as negative variables. We carry out our analyses with variables that measure the *net* balance of deferred tax assets and liabilities of the firm since the database does not split up the deferred tax numbers into assets and liabilities. It is quite possible that the net numbers contain both assets and liabilities. However, we assume the signs of the net numbers (i.e., assets or liabilities) reflect the relative importance of the determinants of deferred taxes.

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[9] Our power of test might also be greater in the U.K. context since previous research argues that in the U.K. regulatory and litigation costs are lower than in the U.S. (see Ball et al. 2000). Previous empirical research documents earnings management practices in the U.K. Most notably, Peasnell et al. (2000) present evidence that, although the so-called Cadbury Report—a voluntary Code of Best Practice issued in 1992 by the Committee on the Financial Aspects of Corporate Governance comprised of the Financial Reporting Council, the London Stock Exchange, and representatives of the accounting profession—has altered the U.K. regulatory framework of corporate governance, these changes did not eliminate earnings management activity by U.K. firms. The results in Peasnell et al. (2000) also suggest board composition influences the occurrence of earnings management in the U.K.

[10] Datastream International collecting the net deferred tax assets/liabilities is consistent with reporting practice in the U.K. Unlike in the U.S. where companies can report deferred tax assets separately from deferred tax liabilities on the balance sheet, companies in the U.K. report the net deferred tax asset or liability.

[11] We identify outliers by plots of each dependent variable against each independent variable. In addition, we delete observations with an absolute value of the r-student larger than 3 in each regression. As a check, we also delete the top and bottom 1 percent of each variable. Results are qualitatively unchanged. Missing variables and outliers in Table 1 relate to test of firm-specific characteristics. The number of observations is smaller in other tests primarily due to missing data items.

TABLE 1
Sample Selection and U.K. Deferred Taxation Activity

Panel A: Sample Selection

	1993	1994	1995	1996	1997	1998	Total
Firm-year observations on Datastream International	931	1,006	1,145	1,295	1,427	1,489	7,293
Missing variables	404	460	528	540	523	473	2,928
Change year-end	12	6	16	24	29	55	142
Outliers	22	24	42	45	77	101	311
Sample	493	516	559	686	798	860	3,912

Panel B: Distribution of Unrecognized and Recognized Net Deferred Taxes

		Unrecognized Deferred Taxes						
		Zero		Asset		Liability		
		RDT	*UDT*	*RDT*	*UDT*	*RDT*	*UDT*	Total
Recognized Deferred Taxes								
Zero	n	381		90		254		725
	Mean	0	0	0	−0.087	0	0.077	
	Std. Dev.	0	0	0	0.118	0	0.101	
	Median	0	0	0	−0.031	0	0.043	
Asset	n	426		135		788		1,349
	Mean	−0.010	0	−0.010	−0.024	−0.010	0.039	
	Std. Dev.	0.012	0	0.011	0.067	0.012	0.064	
	Median	−0.006	0	−0.006	−0.008	−0.006	0.019	
Liability	n	534		94		1,210		1,838
	Mean	0.030	0	0.021	−0.039	0.021	0.041	
	Std. Dev.	0.050	0	0.040	0.063	0.035	0.069	
	Median	0.012	0	0.008	−0.012	0.009	0.020	
Total		1,341		319		2,252		3,912

We collect the sample from Datastream International.

Variable definitions: *RDT* is recognized net deferred taxes; *UDT* is unrecognized net deferred taxes. We scale both *RDT* and *UDT* by contemporaneous market value of the firm. Note that we code assets (liabilities) as negative (positive) variables.

The descriptive statistics in Panel B of Table 1 lead to three observations. First, liabilities form the majority of both *UDT* and *RDT* observations (57.6 percent and 47.0 percent of the samples, respectively). Second, the median absolute values of net deferred tax liabilities (recognized and unrecognized) are generally larger than the corresponding medians of net deferred tax assets. For example, for firms that report no recognized deferred taxes, the medians of unrecognized net deferred tax assets and net liabilities are –0.031 and 0.043, respectively. Finally, net recognized amounts are generally smaller than net unrecognized amounts. For example, firms with both recognized and unrecognized net deferred tax liabilities report a median unrecognized amount of 0.020 versus a recognized amount of 0.009.

V. DEFERRED TAXATION AND FIRM-SPECIFIC FACTORS

Research Design

Our first analysis evaluates whether real operational factors and/or proxies for opportunistic behavior relate to levels and changes of unrecognized deferred taxes by estimating two regressions of the following general form:

$$UDT_t = \alpha_0 + \Sigma_p \alpha_p OPERATIONAL_{pt} + \Sigma_q \alpha_q OPPORTUNISTIC_{qt} + e_{1t} \tag{1a}$$

$$\Delta UDT_t = \beta_0 + \Sigma_p \beta_p \Delta OPERATIONAL_{pt} + \Sigma_q \beta_q \Delta OPPORTUNISTIC_{qt} + e_{2t} \tag{1b}$$

where UDT_t is the balance of net unrecognized deferred tax reported in the notes; $OPERATIONAL_{pt}$ and $OPPORTUNISTIC_{qt}$ refer to operational and opportunistic determinants of deferred taxes; Δ is the change operator; e_{1t} and e_{2t} are error terms. Our ***null*** hypothesis is that if management considers *only* operational variables to distinguish between recognized and unrecognized deferred taxes, then no opportunistic variables will be significant in the model. Conversely, our specification allows us to test if opportunism determines deferred taxes while controlling for the operations of the firm. We measure our proxies for management opportunism such that they are not affected by the measurement of recognized or unrecognized deferred taxes.

We first estimate Equation (1a) in the pooled sample as a benchmark. Next, we examine the relation in subsamples of firms that report net unrecognized deferred tax assets or liabilities as a sensitivity check since we expect determinants of unrecognized deferred tax assets or liabilities to differ. We select our proxies for operational factors based on the general formulation of SSAP No. 15 and on common factors that give rise to deferred taxes in the U.K.[12] We estimate Equation (1b) in the pooled sample only because the distinction between net deferred tax assets or liabilities is less important in the change specification. For instance, a decrease of unrecognized deferred tax assets and an increase of unrecognized deferred tax liabilities are both a *positive* change of unrecognized deferred taxes.[13] Since both types of changes have a similar impact on the balance sheet and income statement, we examine the total change only.[14] Finally, similar to Miller and Skinner (1998), we consider two generic opportunistic reporting strategies, one focusing on minimizing leverage and one focusing on smoothing income.

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[12] We identify the most common operational factors through an examination of detailed footnote disclosure on deferred taxation.

[13] Datastream International (DI) contains the current period recognized deferred tax expense, ΔRDT, which firms report in financial statements. DI does not report the current period unrecognized deferred tax expense, ΔUDT, nor did firms consistently report it in the detailed sample of footnotes that we examined. Therefore, we estimate ΔUDT as the annual change in the balance of *UDT*.

[14] The U.K. statutory tax rate decreased from 33 percent to 31.5 percent in 1997 and to 31.0 percent in 1998. We reestimated all analyses adjusting the change in unrecognized and recognized deferred taxes for these tax rate changes. Results do not significantly change.

Operational Factors

Presence of losses. Similar to the U.S., net losses in the U.K. give rise to tax loss carryforwards. By reporting an unrecognized deferred tax asset for the loss carryforward, the firm signals it expects not to be able to realize the future tax benefit related to the loss carryforward (e.g., because the firm expects continuing losses). Miller and Skinner (1998) document the level of the tax loss carryforward is the most important determinant of the valuation allowance for deferred tax assets in their U.S. sample. We therefore include a variable that captures whether firms have realized accounting losses in both Equations (1a) and (1b). We expect a negative relation between losses and levels or changes of unrecognized deferred tax assets and no relation between losses and levels or changes of unrecognized deferred tax liabilities. We operationalize the presence of losses by defining an indicator variable equaling 1 if the firm reports an accounting loss anytime during the sample period, and 0 otherwise.[15] We use accounting losses to define our variable since Datastream International does not contain information on tax losses.

Advance corporation tax. Advance corporation tax (ACT) is a form of tax imputation and a major factor that gives rise to deferred tax assets. When a firm pays a dividend, it is required to prepay the shareholders' tax on the dividend. As the shareholders declare and pay their tax on the dividend, the firm receives a credit from the taxing authority. The firm's prepayment of taxes on dividends can be offset against taxes payable on income of succeeding accounting periods (Davies et al. 1997, 1235). SSAP No. 15 specifies ACT should be carried forward to the extent that the firm foresees sufficient tax will be assessed on income of the succeeding accounting period, against which the ACT is available for offset. SSAP No. 15 further states ACT should be written off unless their recovery is assured beyond reasonable doubt. In other words, if management believes taxes on future profit will not be sufficient to offset the prepaid dividend tax, then it should not recognize the ACT on the balance sheet.[16] Since ACT arises because of dividends, we include a variable that captures dividend payout: $DIVPAID_t$ is dividends paid in year t scaled by net income before taxes in year t. *Ceteris paribus*, we expect to see higher unrecognized deferred tax assets when the firm pays out more dividends. We also include $\Delta DIVPAID_t$ in the Change Regression (1b) and similarly expect a positive relation with changes of unrecognized deferred taxes.

Capital intensity. Similar to the U.S., the U.K. allows firms to take accelerated depreciation expense for tax purposes. We therefore expect firms to report relatively more unrecognized deferred tax liabilities related to depreciation if they are capital intensive (see

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[15] In the U.K., there is a three-year carryback period and an indefinite carryforward period on losses (Price Waterhouse World Firm Services 1994, 184). We use the presence of a loss anytime during the sample period to capture not only the fact that a net loss gives rise to a loss carryforward but also that managements' expectation of the occurrence of future losses guides the decision to classify the loss carryforward as a recognized or unrecognized deferred tax asset. Although the particular definition of the proxy potentially leads to a low power test, defining the loss indicator variable on an annual basis also has its problems since a loss in a particular year does not lead to contemporaneous deferred tax assets if the firm uses the loss for a tax carryback.

[16] If firms do not expect to realize the ACT in the near future, then they could, of course, also cut or eliminate the dividend. However, the finance literature documents dividends are generally "sticky" and managers exhibit a reluctance to eliminate dividends after long dividend histories (see for example DeAngelo and DeAngelo 1990).

Citron 2001). We measure capital intensity (*CAPINT*) as the proportion of net plant and equipment to total assets.[17] We include *CAPINT* in the levels specification and expect a positive relation with unrecognized deferred tax liabilities and no relation with unrecognized deferred tax assets. We include Δ*CAPINT* in the changes specification and expect a positive coefficient.

Asset revaluations. In the U.K., asset revaluations also give rise to deferred taxes. When U.K. managers revalue an asset upward, the recognized gain potentially leads to a recognized deferred tax liability. If management has no intention to sell the asset within the foreseeable future, then the gain will not be realized and therefore the deferred tax liability need not be recognized.[18] We therefore expect that asset revaluations relate positively to unrecognized deferred tax liabilities. We estimate the amount of asset revaluations (*REVSHE*) as the revaluation reserve over shareholders' equity. We include Δ*REVSHE* in the changes specification and expect a positive relation with Δ*UDT*.

Opportunistic Factors

Leverage hypothesis. Managers of levered firms are concerned with violating the restrictions imposed by accounting-based debt covenants. Previous research shows leverage ratios serve as reasonable proxies for the proximity to loan restrictions: book value of debt under GAAP is frequently restricted in public agreements and companies with higher leverage ratios are closer than others to their debt covenant restrictions.[19] Using the partial provision method, managers can influence leverage ratios by reporting relatively higher unrecognized deferred tax liabilities, given a total amount of deferred tax liabilities. *Ceteris paribus*, higher unrecognized deferred tax liabilities reduce the numerator and/or increase the denominator in traditional leverage ratios (e.g., debt-equity ratios). Thus, we hypothesize leverage will be positively related to *unrecognized* deferred tax liabilities if management uses the measurement of deferred taxes to opportunistically manage leverage indicators. In the case of potential deferred tax assets, management can choose to report relatively more recognized and less unrecognized deferred tax assets to increase the denominator in traditional leverage ratios. However, based on Miller and Skinner's (1998) evidence on the link between leverage and the valuation allowance in U.S. GAAP, we expect the relation between unrecognized deferred tax assets and leverage to be weaker. We define leverage (*LEV*) as total short-term plus long-term debt, scaled by shareholders' equity of the firm adjusted for unrecognized deferred taxes.[20] The calculation of deferred taxes, therefore, does not affect our proxy for leverage, allowing it to capture the propensity to manage earnings to influence leverage. We also include Δ*LEV* in the changes specification. Consistent with our hypothesis for the levels specification, we expect the sign of the coefficient to be positive in the change specification.

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[17] We define *CAPINT* using *net* plant and equipment since it is a better measure of the differences between a firm's book and tax depreciation than *gross* plant and equipment. Net plant and equipment reflects future book depreciation of the firm. Notice also that revaluations generally do not distort *CAPINT* since most revaluations pertain to real estate, in particular land (Lin and Peasnell 2000). We reestimate our analyses using a measure of *gross* plant and equipment and the results remain unaltered.

[18] The rule has been made explicit in FRS No. 19. On this issue, FRS No. 19 differs from international practice that states that the deferred tax liability needs to be recognized irrespective of intentions to sell the revalued asset.

[19] For example, Duke and Hunt (1990) and Press and Weintrop (1990).

[20] We therefore include unrecognized taxes in the computation of our ratio, as if they had been recognized. Results also are robust to adjusting leverage and the change in *ROA* to exclude the effects of recognized and unrecognized deferred taxes.

Income smoothing. Management potentially uses its discretion over the amount of recognized versus unrecognized deferred taxes to report earnings opportunistically. Similar to prior research, we focus on whether management uses its discretion to smooth earnings (Schrand and Wong 2002; Miller and Skinner 1998). We use the change in operating income (measured before taxes) deflated by beginning total assets adjusted for deferred taxes (ΔROA) to test whether management determines unrecognized deferred taxes to smooth earnings (see Miller and Skinner 1998). If management wishes to smooth bottom-line earnings in a year when operating earnings decrease (increase), then they could use the *relative* level of unrecognized deferred taxes to do so: relatively higher (lower) unrecognized deferred tax liabilities or relatively lower (higher) unrecognized deferred tax assets increase (decrease) bottom-line earnings. Therefore, we predict that if management uses unrecognized deferred taxes to smooth bottom-line earnings, then the change in unrecognized deferred taxes will be negatively related with our proxy for income smoothing ΔROA since unrecognized deferred tax assets (liabilities) are coded as negative (positive) variables. We implicitly assume managers act opportunistically by choosing the *relative* level of recognized versus unrecognized deferred taxes.

Control Variables

In addition to the opportunistic and operational factors, we include two control variables in the regression: firm size and recognized deferred taxes (*RDT*). We include size as a general control variable because size is associated with a number of factors such as risk, growth, and accounting decisions that potentially determine the level of unrecognized deferred taxes. We make no prediction about the relation between unrecognized deferred taxes and size. We define size as the log of the current period market value of the firm, i.e., $SIZE_t$ equals $\log(market\ value_t)$.

We include recognized deferred taxes in the regression to mitigate a potential correlated omitted variable problem that arises if we do not identify all relevant determinants of unrecognized deferred taxes. While we identify the most common set of determinants that give rise to total deferred taxes, the inclusion of recognized deferred taxes potentially captures those factors we do not explicitly include in the regression. We include change versions of both control variables in Equation (1b).

To summarize, to assess which operational and which opportunistic variables are related to the levels and changes of *UDT*, we estimate the following two regressions:

Levels Regression:

$$\begin{aligned} UDT_t = {} & \alpha_0 + \alpha_1 LOSS_t + \alpha_2 DIVPAID_t + \alpha_3 CAPINT_t + \alpha_4 REVSHE_t \\ & + \alpha_5 LEV_t + \alpha_6 SIZE_t + \alpha_7 RDT_t + e_{1t}. \end{aligned} \tag{2a}$$

Changes Regression:

$$\begin{aligned} \Delta UDT_t = {} & \beta_0 + \beta_1 LOSS_t + \beta_2 \Delta DIVPAID_t + \beta_3 \Delta CAPINT_t + \beta_4 \Delta REVSHE_t \\ & + \beta_5 \Delta LEV_t + \beta_6 \Delta ROA_t + \beta_7 \Delta SIZE_t + \beta_8 \Delta RDT_t + e_{2t}. \end{aligned} \tag{2b}$$

Firm subscripts are omitted. To mitigate scale issues, we divide all deferred tax variables by contemporaneous market value throughout all analyses. To control for the possibility of omitted time-specific macroeconomic effects, we include fiscal-year fixed effects indicator variables; e_{1t} and e_{2t} are error terms.[21]

Results of the Regression Tests of Deferred Taxation and Firms-Specific Factors

Table 2, Panel A, presents descriptive statistics of the variables included in the first tests for the pooled sample and subsamples of observations with *net* unrecognized deferred tax liabilities or assets. The descriptive results for all operational variables, except *DIVPAID*, are consistent with the predictions. Specifically, the unrecognized deferred tax assets (*UDTA*) subsample contains more *LOSS* observations than the unrecognized deferred tax liabilities (*UDTL*) subsample. Also, *UDTL* firms are more capital-intensive and revalue assets more than *UDTA* firms. In terms of the opportunism variables, the differences between both subsamples are less pronounced. Median (but not mean) leverage is higher in the *UDTL* sample and the mean (but not median) ΔROA is higher in the *UDTA* sample. Note finally that *UDTL* firms are larger and report higher recognized deferred taxes than *UDTA* firms. One explanation for the larger mean and median of *DIVPAID* in the *UDTL* sample therefore is that larger firms pay more dividends.

Table 3, Panel A, reports the results relating to the levels regression. The panel first reports the results of the pooled regression and then shows the results of separate regressions in subsamples of net unrecognized deferred tax liabilities and assets observations. Since the pooled regression summarizes the two subsample regressions, we focus our discussion on the two subsample regressions. The panel also shows the prediction of the signs on the coefficients in both regressions.

In the subsample of unrecognized deferred tax liabilities, the coefficient on both operational variables *CAPINT* and *REVSHE* are of the predicted sign and highly significant (0.122 and 0.094 with White [1980] t-statistics of 15.301 and 3.635, respectively). In contrast, we did not expect *LOSS* and *DIVPAID* to relate necessarily to unrecognized deferred tax liabilities and find their coefficients to be insignificant. We also find a coefficient of 0.017 on the leverage variable (significant at the 5 percent level), consistent with the hypothesis that management reports more unrecognized deferred tax liabilities in the presence of higher debt-equity ratios. The coefficient on *RDT* is significantly positive, indicating that firms that report more recognized deferred tax liabilities also report more unrecognized deferred tax liabilities. Since more recognized deferred tax liabilities potentially increase the firm's leverage, the result for *RDT* is consistent with the positive coefficient on the leverage variable and its interpretation that management reports more unrecognized deferred tax liabilities in the presence of higher leverage. Finally, the coefficient on control variable *SIZE* is negative and significant.

In the subsample of unrecognized deferred tax assets we find a significant negative coefficient on *LOSS* (−0.060), indicating the firm reports relatively more unrecognized deferred tax assets when it has incurred losses. As predicted, the coefficient on *DIVPAID* is positive (0.037) and significant at the 1 percent level. Consistent with expectations and in contrast with the results of the regression on unrecognized deferred tax liabilities, we find the coefficients on *CAPINT* and *REVSHE* are not significant. The coefficient on our leverage proxy is also insignificant, suggesting management does not choose unrecognized

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[21] We considered using the ratio of *UDT* to *RDT* or to total deferred taxes as the dependent variable in Equations (2a) and (2b). However, because deferred taxes can be both assets and liabilities, and therefore can be negative or positive, the resulting ratios are not well behaved.

TABLE 2
Descriptive Statistics

	All Observations (n = 2,571)			UDTL Sample (n = 2,252)			UDTA Sample (n = 319)			Probability Values of Tests of Equality of Means and Medians Asset–Liability	
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median	Mean[a]	Median[a]
Panel A: Variables in Levels Tests of Deferred Taxation and Firm-Specific Characteristics											
UDT	0.033	0.080	0.016	0.044	0.072	0.021	−0.047	0.088	−0.012	0.281	0.000
LOSS	0.287	0.452	0.000	0.263	0.440	0.000	0.452	0.498	0.000	0.000	0.000
DIVPAID	0.315	0.619	0.259	0.325	0.645	0.262	0.241	0.373	0.228	0.029	0.015
CAPINT	0.415	0.234	0.366	0.441	0.230	0.394	0.237	0.178	0.190	0.000	0.000
REVSHE	0.061	0.091	0.017	0.064	0.092	0.020	0.039	0.076	0.001	0.000	0.000
LEV	0.253	0.394	0.149	0.253	0.391	0.156	0.253	0.416	0.097	0.990	0.008
ROA	0.028	0.092	0.023	0.025	0.078	0.022	0.042	0.158	0.028	0.002	0.255
SIZE	11.576	2.085	11.417	11.631	2.094	11.428	11.187	1.982	11.227	0.000	0.443
RDT	0.007	0.030	0.000	0.008	0.030	0.001	0.002	0.026	0.000	0.001	0.000
Panel B: Variables in Prediction Tests[b]											
ΔUDT	0.001	0.025	0.001								
ΔRDT	0.001	0.013	0.000								
$\Delta TOTDT_{t+1}$	0.002	0.041	0.001								
$\Delta TOTDT_{t+2}$	0.005	0.060	0.002								
$\Delta TOTDT_{t+3}$	0.010	0.083	0.004								
CFO	0.179	0.165	0.151								
EBITDA	0.205	0.219	0.181								
OPINC	0.138	0.204	0.132								

(continued on next page)

TABLE 2 (continued)

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[a] Probability values presented for *UDT* test the equality of means and medians of the absolute value of *UDT*.

[b] The number of observations used in the prediction tests decreases due to data constraints. The predictability tests of changes in deferred taxes use 2,096 observations. The predictability test of future performance relating cash flow, *EBITDA* and operating income, uses 1,512, 1,867, and 1,595 observations, respectively.

Variable definitions:

UDT = unrecognized net deferred taxes divided by contemporaneous market value;
RDT = recognized net deferred taxes divided by contemporaneous market value;
LOSS = a bivariate variable taking the value of 1 if the firm reported an accounting loss during the sample period, and 0 otherwise;
DIVPAID = dividends paid divided by net income before taxes;
CAPINT = net plant and equipment divided by total assets;
REVSHE = revaluation reserve divided by shareholders' equity;
LEV = leverage calculated as the sum of short-term plus long-term debt divided by shareholders' equity adjusted for unrecognized deferred taxes;
ΔROA = the change in operating income divided by the beginning balance of total assets adjusted for unrecognized deferred taxes;
SIZE = the natural logarithm of market value;
ΔUDT = the change in unrecognized net deferred taxes divided by beginning market value;
ΔRDT = change in recognized net deferred taxes divided by beginning market value;
$\Delta TOTDT_{t+n}$ = the change in total (recognized plus unrecognized) deferred taxes divided by beginning market value where n = 1,2,3;
CFO = cash flows from operations divided by beginning market value;
EBITDA = earnings before interest, taxes and depreciation expense divided by beginning market value; and
OPINC = operating income divided by beginning market value.

TABLE 3
Regression Tests of Deferred Taxation and Firm-Specific Characteristics

Panel A: Levels Specification Equation (2a)

$$UDT_t = \alpha_0 + \alpha_1 LOSS_t + \alpha_2 DIVPAID_t + \alpha_3 CAPINT_t + \alpha_4 REVSHE_t + \alpha_5 LEV_t + \alpha_6 SIZE_t + \alpha_7 RDT_t + e_t\,^a \tag{2a}$$

	INT.	*LOSS*	*DIVPAID*	*CAPINT*	*REVSHE*	*LEV*	*SIZE*	*RDT*	Adj. R^2	F-value (prob.)
Pooled sample of unrecognized deferred taxes and liabilities (2,571 Obs.)										
Coefficient	0.019	−0.014	0.007	0.129	0.095	0.016	−0.005	0.217	0.233	65.13
White t-stat	1.786	−3.824***	2.082**	16.096***	3.551***	2.641***	−4.872***	2.508**		(0.000)
Sample of unrecognized deferred tax liabilities (2,252 Obs.)										
Sign?	?	?	?	+	+	+	?	?		
Coefficient	0.059	0.005	0.003	0.122	0.094	0.017	−0.007	0.198	0.257	65.01
White t-stat	5.726**	1.641	1.042	15.301***	3.635***	2.348**	−7.241***	2.128**		(0.000)
Sample of unrecognized deferred tax assets (319 Obs.)										
Sign?	?	−	+	?	?	+	?	?		
Coefficient	−0.134	−0.060	0.037	0.011	−0.003	0.006	0.009	−0.090	0.232	8.96
White t-stat	−5.309**	−6.692***	2.764***	0.433	−0.035	0.793	4.603***	−0.829		(0.000)

Panel B: Changes Specification Equation (2b)

$$\Delta UDT_t = \beta_0 + \beta_1 LOSS_t + \beta_2 \Delta DIVPAID_t + \beta_3 \Delta CAPINT_t + \beta_4 \Delta REVSHE_t + \beta_5 \Delta LEV_t + \beta_6 \Delta ROA_t + \beta_7 \Delta SIZE_t + \beta_8 \Delta RDT_t + e_t\,^a \tag{2b}$$

	INT.	*LOSS*	*ΔDIVPAID*	*ΔCAPINT*	*ΔREVSHE*	*ΔLEV*	*ΔROA*	*ΔSIZE*	*ΔRDT*	Adj. R^2	F-value (prob.)
Pooled sample of unrecognized deferred tax liabilities and assets (2,472 Obs.)											
Sign?	?	−	+	+	+	+	−	?	?		
Coefficient	−0.003	−0.006	0.020	0.046	0.027	0.004	0.018	−0.001	0.030	0.073	15.84
White t-stat	−1.718	−3.720***	2.934***	5.348***	1.556	2.331**	1.545	−0.487	0.401		(0.000)

(*continued on next page*)

TABLE 3 (continued)

***, ** denote significance at the 0.01 and 0.05 levels, respectively.

[a] Fiscal-year fixed-effects indicator variables are included in the equation but not presented. Firm subscripts are omitted.

Variable definitions:
UDT = unrecognized net deferred taxes divided by market value;
$LOSS$ = a bivariate variable taking the value of 1 if the firm reported a loss during the sample period, and 0 otherwise;
$DIVPAID$ = dividends paid divided by net income before taxes;
$CAPINT$ = net plant and equipment divided by total assets;
$REVSHE$ = revaluation reserve divided by shareholders' equity;
LEV = leverage calculated as the sum of short-term plus long-term debt divided by shareholders' equity adjusted for unrecognized deferred taxes;
ΔROA = the change in operating income divided by the beginning balance of total assets adjusted for unrecognized deferred taxes;
$SIZE$ = the natural logarithm of market value; and
RDT = recognized net deferred taxes divided by market value.

Δ is the change operator. Firm and time subscripts are omitted.

deferred tax assets to improve its leverage indicators. Note that consistent with the insignificant coefficient on leverage, the coefficient on *RDT* is also not significant in the subsample of unrecognized deferred tax assets. Last, we observe the coefficient on *SIZE* is positive (0.009) and significant at the 1 percent level.

We report the results of the change regression in Panel B of Table 3. The panel shows the coefficients on the operational factors *LOSS*, Δ*DIVPAID*, and Δ*CAPINT* have the expected signs and are significant. The coefficient on Δ*REVSHE*, although of the correct sign, is not significant. Focusing on the opportunistic variables, we find the coefficient on the changes in leverage (Δ*LEV*) has the correct sign and is significant, consistent with the results of the levels regressions in Panel A. In contrast, we find no evidence for Δ*ROA*: the coefficient has the wrong sign and is not significant. Finally, the coefficients on both control variables are also insignificant. The results of the changes regression in Panel B therefore are consistent with the effect of leverage on the measurement of deferred taxes shown in Panel A, but show no evidence U.K. managers measure deferred taxes to smooth earnings.

Sensitivity Tests Related to Opportunism

We test the sensitivity of the results relating to leverage by focusing on two groups of firms more likely to report unrecognized deferred taxes opportunistically. To form our groups, we rely on the subsamples of firms in Table 1. In the subsample of firms reporting net unrecognized deferred tax liabilities, we expect to find stronger evidence of opportunism to reduce leverage when firms report net recognized deferred tax assets or zero recognized deferred taxes rather than recognized deferred tax liabilities. Firms more concerned about managing leverage potentially avoid reporting additional liabilities by keeping all of their deferred tax liability off the balance sheet. Similarly, in the subsample of firms with net unrecognized deferred tax assets, we expect firms with net recognized deferred tax assets to likely report more opportunistically than firms with recognized deferred tax liabilities or zero recognized deferred taxes. Generally, firms that report unrecognized deferred tax assets and recognized deferred tax liabilities opt for a combination of unrecognized and recognized deferred taxes with the most negative effect on leverage. Therefore, if the firm with unrecognized net deferred tax assets is concerned about the leverage increasing effects of recognized deferred taxes, it will report net recognized deferred tax assets to avoid recording any deferred tax liabilities on the balance sheet.

To test our refined hypothesis, we augment Regression (2a) with interactive variables and indicator intercept terms. In the sample of firms with net unrecognized deferred tax *liabilities*, we interact leverage with an indicator variable equaling 1 if the firm reports recognized deferred tax liabilities, and 0 otherwise. We predict a negative coefficient on the interaction variable since the indicator variable pertains to firms we *least* expect to report opportunistically in the subsample of net unrecognized deferred tax liabilities. In the sample of firms with unrecognized deferred tax *assets*, we interact leverage with an indicator variable equaling 1 if the firm reports recognized deferred tax assets, and 0 otherwise. Here, we predict a positive coefficient on the interaction variable since the indicator variables captures firms we *most* expect to report opportunistically in the sample of firms with net unrecognized deferred tax assets.

Table 4 reports the results of the analysis in both subsamples. In the subsample of firms with net unrecognized deferred tax liabilities, the coefficient on leverage (*LEV*) is 0.033 with a White t-statistic of 4.227. In addition, the coefficient on the interaction variable is significantly negative (−0.029, White t-statistic of −3.467), suggesting firms with both unrecognized and recognized deferred tax liabilities are *less* likely to report deferred taxes

TABLE 4
Regression Tests of Deferred Taxation and Firm-Specific Characteristics: Sensitivity Tests

Levels Specification Equation (2a)[a]

Observations with unrecognized deferred tax liabilities

	INT.	*LOSS*	*DIV-PAID*	*CAPINT*	*REV-SHE*	*LEV*	*LEV* RDTLDUM*	*RDTLDUM*	*SIZE*	*RDT*	Adj. R^2	F-value (prob.)
Sign?		?	?	?	+	+	−	?	?	?		
Coefficient	0.061	0.005	0.002	0.122	0.079	0.033	−0.029	−0.014	−0.007	0.351	0.266	57.13
White t-stat	6.020***	1.671	0.807	15.105***	3.087***	4.227***	−3.467***	−3.779***	−6.971***	3.001***		(0.000)

Observations with unrecognized deferred tax assets

	INT.	*LOSS*	*DIV-PAID*	*CAPINT*	*REV-SHE*	*LEV*	*LEV* RDTADUM*	*RDTDUM*	*SIZE*	*RDT*	Adj. R^2	F-value (prob.)
Sign?		−	+	?	?	+	+	?	?	?		
Coefficient	−0.141	−0.055	0.027	0.021	−0.023	0.000	0.011	0.015	0.009	0.021	0.228	7.45
White t-stat	−5.425***	−6.216***	2.594***	0.898	−0.319	−0.020	0.835	1.360	4.552***	0.223		(0.000)

***, ** denote significance at the 0.01 and 0.05 levels, respectively.

[a] Regression (2a) (see Table 3, Panel A) is augmented with an interactive variable of *LEV* and an indicator variable.

In the sample with unrecognized deferred tax liabilities observations, this indicator variable (*RDTLDUM*) equals 1 if a firm reports both unrecognized and recognized deferred tax liabilities, and 0 otherwise.

In the sample with unrecognized deferred tax asset observations, this indicator variable (*RDTADUM*) equals 1 if a firm reports both unrecognized and recognized deferred tax assets, and 0 otherwise.

See Table 2 for the specification of Regressions (2a) and (2b) and variable definitions.

opportunistically; the sum of both coefficients is not significantly different from zero indicating there is no association of leverage with unrecognized deferred taxes for firms in this partition of the sample.

In the net unrecognized deferred tax assets subsample, we find not only that the coefficient on leverage (*LEV*) remains insignificant, but also that the coefficient on the interaction variable is insignificant. Similar to Miller and Skinner (1998), we therefore find management does not measure unrecognized deferred tax assets opportunistically to influence leverage indicators.

We also perform three sensitivity tests of the changes specification to focus on earnings management.[22] Since the results of these tests are consistent with our finding of no opportunism relating to earnings management, we do not report the results but rather discuss them briefly. In the first sensitivity test of earnings management, we interact ΔROA with indicator variables equaling 1 if the firms falls in the highest (lowest) decile of the ΔROA distribution, and 0 otherwise.[23] We expect a larger change of *ROA* to indicate more opportunity for opportunistic behavior, but find none of the coefficients on the interactive variables are significant, consistent with changes in return on assets not being related to changes in unrecognized deferred taxes.

The absence of support for the smoothing hypothesis potentially follows either from our choosing a weak proxy for earnings smoothing or from U.K. managers not engaging in earnings smoothing through the use of deferred taxes. To differentiate between both possibilities, we estimate a second sensitivity test substituting ΔROA with an earnings management variable, *EMT*, defined as an indicator variable taking the value of 1 if a firm's change in earnings is between 0 and 0.01 percent of market value, and 0 otherwise. Following Burgstahler and Dichev (1997), we assume *EMT* identifies potential earnings management situations.[24] Peasnell et al. (2000) use a similar earnings management variable in a U.K. context and find it detects instances of earnings management. If *EMT* detects potential earnings management situations, a positive coefficient on *EMT* would suggest management boosted earnings by an increase (decrease) in unrecognized deferred tax liabilities (assets). Untabulated results however show the coefficient on *EMT* is not significant.

Last, we construct a measure of management of pretax earnings to focus on a situation where deferred taxes are the earnings management tool of last resort. If a firm experiences a small decline in pretax earnings during the year, then management could artificially increase bottom-line earnings through the deferred tax expense. We therefore reestimate Equation (2b) substituting ΔROA with a pretax earnings management variable, *PTEMT*, defined as an indicator variable taking the value of 1 if the firm's change in pretax earnings is between −0.01 and 0 percent of market value, and 0 otherwise.[25] If *PTEMT* detects potential earnings management situations, a positive coefficient on *PTEMT* would suggest management boosted earnings by an increase (decrease) in unrecognized deferred tax liabilities (assets). Untabulated results again show the coefficient on *PTEMT* is not significant.

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[22] We believe the changes specification does not lend itself well for an additional sensitivity test of the leverage hypothesis. We mention earlier that Datastream International reports only *net* numbers for recognized and unrecognized deferred taxes and therefore we cannot trace to what extent a particular change in unrecognized deferred taxes represents a change in *UDTL* or *UDTA*. It is therefore impossible to formulate predictions based on whether the unrecognized or recognized deferred taxes are assets or liabilities as in the levels regressions.

[23] The highest and lowest groups represent the top and bottom decile of the change in *ROA*. Results are similar when we use a cut-off of 5 percent and 15 percent.

[24] The change in earnings is between 0 and 0.01 percent of market value for approximately 15 percent of the observations.

[25] The change in pretax earnings is between −0.01 and 0 percent of market value for approximately 10 percent of the observations. Results are robust when we examine smaller pretax changes in earnings (between −0.005 and 0 percent of market value and between −0.001 and 0 percent of market value).

To summarize, our first set of results indicate levels and changes of unrecognized deferred taxes relate predictably to operational factors. We also find evidence that levels of unrecognized deferred taxes relate to one opportunistic factor, namely leverage, for a subset of firms with unrecognized deferred tax liabilities. Consistent with the U.S.-based results in Miller and Skinner (1998), we find no evidence of opportunism in the measurement of unrecognized deferred tax assets. Additionally, we find no evidence that changes in unrecognized deferred taxes relate to proxies for earnings smoothing or earnings management in general.

VI. DEFERRED TAXATION, FUTURE REVERSALS, AND FUTURE PROFITABILITY

Although the previous results suggest management opportunism potentially determines recognized and unrecognized deferred taxes, they depend, however, on our choices of operational and opportunistic determinants of deferred taxes. Therefore, to complement our first set of results, we next examine whether the components of deferred taxes forecast future reversals of deferred taxes or future profitability differently, consistent with management's choice of recognized versus unrecognized deferred taxes.

Predicting Future Deferred Tax Reversals and Future Profitability: Research Design

To implement the partial method, management first forecasts *total* timing differences between tax and book income over the next few years. At any given time, management recognizes deferred taxes only if the level of total timing differences decreases (i.e., reverses) in the foreseeable future. As a result, if management successfully predicts deferred tax reversals and if management truthfully measures recognized deferred taxes, then changes in recognized deferred taxes will be *negatively* related to future changes in total deferred taxes. Similarly, changes in *unrecognized* deferred taxes will be *positively* related to future changes in total deferred taxes.[26]

To investigate whether management truthfully reports deferred taxes, we examine the relation between changes in recognized and unrecognized deferred taxes and future reversals up until three years in the future. We select the three-year horizon because SSAP No. 15 identifies three to five years as the time period over which recognized deferred taxes should crystallize, i.e., reverse.[27] We therefore estimate the following regression:

$$\Delta TOTDT_{t+n} = \varphi_0 + \varphi_1 \Delta RDT_t + \varphi_2 \Delta UDT_t + \varphi_3 RDT_t + \varphi_4 UDT_t + \varphi_5 SIZE_t + \varphi_6 MTB_t + u_t. \tag{3}$$

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[26] To clarify: consider timing differences related to depreciation. When management plans to make capital investments in the foreseeable future, it will typically forecast an increase in cumulative timing differences related to depreciation. As long as management forecasts increases in cumulative timing differences because of the investment program, it will not recognize any (additional) deferred tax liabilities related to depreciation, but instead measure only unrecognized deferred tax liabilities related to depreciation. If management foresees cumulative timing differences will start decreasing in the foreseeable future, say, because it scales down its future capital investment programs, then it will increase (decrease) recognized (unrecognized) deferred tax liabilities to reflect the decrease in forecasted total timing differences related to depreciation (for more details, see Davies et al. 1997, Chapter 21).

[27] Data limitations preclude us from extending the tests beyond three years.

Firm subscripts are omitted. $\Delta TOTDT_{t+n}$ is the *cumulative* change in future total deferred taxes (i.e., recognized plus unrecognized deferred taxes) over the forecast horizon (n = 1,2,3). The main variables of interest are the change in recognized deferred taxes (ΔRDT) and the change in unrecognized deferred taxes (ΔUDT). We also include the level of both recognized (RDT) and unrecognized (UDT) deferred taxes in the equation since they potentially capture part of the future reversals recorded in previous years. As in Equations (2a) and (2b), we include size as a control variable. Since the prediction tests aim to establish if management signals its private information through the measurement of the components of deferred taxes, we also incorporate the market-to-book ratio (MTB_t) to capture growth opportunities already priced by the market. u_t is an error term.

In a second test we investigate the relation between the components of deferred taxes and future firm profitability. The underlying factors associated with unrecognized deferred taxes likely also relate to future profitability. For example, a capital-intensive firm that continues to invest in depreciable assets will do so presumably because the investments are profitable. If a firm continues to invest in capital assets, then its deferred tax liability relating to depreciation will not reverse. Therefore, we expect the unrecognized deferred tax liability to be associated with future profitability because the underlying depreciable assets used to produce the future profits determine it. Managers, then, can signal their private information about future firm profitability by using the distinction between recognized and unrecognized deferred taxes. Citron (2001) examines the signaling hypothesis indirectly by estimating a market-based test and finds investors interpret deferred taxes as if they convey information about future growth of the firm through capital spending. We extend his work by examining directly whether the components of deferred taxes forecast future firm profitability differently. By keeping deferred tax liabilities related to depreciation timing differences off the balance sheet and, thus, recording more unrecognized deferred tax liabilities, management can signal their belief the firm will continue to invest and grow (see Citron 2001). In contrast, if management thinks the firm will *not* realize certain future tax benefits (e.g., the firm will not become profitable soon enough to benefit from loss carryforwards), then they can decide to keep deferred tax assets off the balance sheet (i.e., a negative change of unrecognized deferred taxes). To test our hypothesis, we estimate the following regressions:

$$PERF_{t+n} = b_0 + b_1 PERF_t + b_2 \Delta RDT_t + b_3 \Delta UDT_t + b_4 SIZE_t + b_5 MTB_t + u_t \tag{4}$$

where $PERF$ is one of three alternative performance measures used to ensure robustness, namely cash flow from operations (CFO), earnings before interest, taxes, depreciation and amortization ($EBITDA$), and operating income ($OPINC$). We calculate each profitability measure before income tax expense to eliminate any mechanical relation between our profitability measures and deferred taxes. We include cash flow from operations as one of our profitability measures since it is not affected by accrual accounting choices that potentially influence the association we examine. Firm subscripts are omitted and n = 1,2,3. u_t is an error term. Our hypothesis implies that if management signals future performance through the unrecognized component of deferred taxes, then the coefficient b_2 on ΔUDT_t will be positive.

Empirical Findings of the Prediction Tests

Table 2, Panel B, presents descriptive statistics of additional variables included in the prediction tests. The means and medians of change in recognized and unrecognized deferred taxes scaled by market value (ΔRDT and ΔUDT) are both positive; also unreported tests indicate they do not differ significantly. In approximately 20 percent (9 percent) of observations, ΔRDT (ΔUDT) is zero. Panel B also reports the increase in the cumulative change in total deferred taxes over the one-, two-, and three-year horizon.

Table 5 reports our findings on the relation between deferred taxes and future deferred tax reversals. As a reminder, the dependent variable in each equation is the *cumulative* change in total deferred taxes in the future since current changes in deferred taxes could forecast reversals over a number of years into the future. We find the coefficients on changes in recognized and unrecognized deferred taxes are both statistically insignificant in the first equation ($t+1$). In contrast, the results in the second and third equation focusing on *cumulative* future change in total deferred taxes exhibit the predicted pattern. In both cases, the coefficient on the changes of recognized (unrecognized) deferred taxes is reliably negative (positive): both coefficients are significant at the 10 percent level in the second equation and at the 5 percent level in the third equation.[28] The absolute magnitude of the coefficients also increases as we move from the first to the third equation, confirming the predictive power of the components of deferred taxes over the forecast horizon. The deferred tax levels variables and *MTB* are never statistically significant, whereas *SIZE* is significant in all three equations (at different significance levels). To summarize, we find evidence consistent with management measuring deferred taxes *as if* they forecast future reversals of timing difference. The results are strongest in the third equation indicating management looks at least three years ahead in determining recognized and unrecognized deferred taxes.

Table 6 shows the results of our prediction tests of future profitability. In general, we find changes in unrecognized deferred taxes strongly predict future profitability proxies. In each regression, the coefficient on changes in unrecognized deferred taxes is positive and significant.[29] The results, therefore, confirm Citron's (2001) findings and suggest management signals future profitability of the firm using unrecognized deferred taxes. With two exceptions, changes in recognized deferred taxes do *not* predict future profitability, thus strengthening our interpretation of the results.[30]

To evaluate whether potential balance sheet management influences the prediction tests, we reestimate Equations (3) and (4) including additional terms capturing the interaction between leverage and changes in recognized and unrecognized deferred taxes.[31] Unreported analyses find no significant coefficients on these interaction variables, suggesting that the potential existence of balance sheet management documented earlier does not invalidate the predictive power of unrecognized deferred taxes for future deferred tax reversals and profitability measures.

Summarizing, our prediction tests provide no evidence of opportunism driving the measurement of unrecognized deferred taxes in the U.K. Although U.K. managers potentially can systematically manipulate our proxies for future profitability, we do not expect this

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[28] Results are robust to excluding observations with either zero ΔRDT or ΔUDT.

[29] Results are again robust to excluding observations with either zero ΔRDT or ΔUDT. Results are consistent but weaker when we examine the change in the performance measures.

[30] The two exceptions are the $t+1$ *EBITDA* and *OPINC* regressions showing significant *negative* coefficients on changes of recognized deferred taxes. The finding is consistent with our interpretation of the coefficients on the changes in unrecognized deferred taxes, if additional recognition of deferred taxes implies a decrease in unrecognized deferred taxes.

[31] For the purpose of carrying out this test, we estimate change of leverage in different ways, either through indicator variables that identify high versus low leverage firms or through the change in debt-to-equity ratio, a continuous variable. All results are qualitatively equivalent.

TABLE 5
Regression Results Relating to Predictability Tests of Changes in Deferred Taxes

$$\Delta TOTDT_{t+n} = \varphi_0 + \varphi_1 \Delta RDT_t + \varphi_2 \Delta UDT_t + \varphi_3 RDT_t + \varphi_4 UDT_t + \varphi_5 SIZE_t + \varphi_6 MTB_t + u_t{}^{a} \quad (3)$$

(n = 2,093)	$\Delta TOTDT_{t+n}$	INT.	ΔRDT	ΔUDT	RDT	UDT	$SIZE$	MTB	Adj. R^2	F-value (prob.)
Expected Sign		?	−	+	?	?	?	?		
Coefficient	$t+1$	−0.006	0.046	0.011	−0.047	−0.016	0.001	0.000	0.008	2.75
White t-stat		−0.921	1.017	0.151	−1.245	−0.569	2.221**	−1.284		(0.002)
Coefficient	$t+2$	−0.006	−0.393	0.223	−0.080	−0.010	0.001	0.000	0.024	6.22
White t-stat		−0.751	−1.932	1.918	−1.661	−0.284	1.954	−1.273		(0.000)
Coefficient	$t+3$	−0.012	−0.490	0.330	−0.027	0.001	0.002	−0.001	0.023	5.84
White t-stat		−1.314	−2.356**	2.194**	−0.329	0.018	3.077***	−1.312		(0.000)

***, ** denote significance at the 0.01 and 0.05 levels, respectively.

[a] Fiscal-year fixed-effects indicator variables are included in the equation but not presented. Firm subscripts are omitted.

Variable definitions:

$\Delta TOTDT_{t+n}$ = the change in total (recognized plus unrecognized) deferred taxes where n = 1,2,3;
RDT = recognized net deferred taxes;
UDT = unrecognized net deferred taxes;
$SIZE$ = the natural logarithm of market value; and
MTB = the market-to-book ratio at time t.

Δ is the change operator. $\Delta TOTDT$, ΔRDT, ΔUDT, RDT, and UDT are divided by beginning market value.

TABLE 6
Regression Results Relating to Predictability Tests of Future Performance

$$PERF_{t+n} = b_0 + b_1 PERF_t + b_2 \Delta RDT_t + b_3 \Delta UDT_t + b_4 SIZE_t + b_5 MTB_t + u_t^a \qquad (4)$$

Dependent Variable		INT.	*PERF*	*ΔRDT*	*ΔUDT*	*SIZE*	*MTB*	Adj. R^2	F-value (prob.)
Expected Sign		?	+	−	+	?	?		
CFO	(n = 1,512)								
Coefficient	*t*+1	0.181	0.624	−0.718	0.765	−0.007	0.001	0.366	97.85
White t-stat		6.193***	9.443***	−1.239	2.742***	−3.320***	0.393		(0.000)
Coefficient	*t*+2	0.225	0.766	1.372	0.942	−0.010	0.000	0.344	92.45
White t-stat		6.417***	11.797***	1.775	4.056***	−4.373***	0.242		(0.000)
Coefficient	*t*+3	0.297	0.760	−0.936	1.505	−0.015	0.001	0.280	69.04
White t-stat		6.833***	7.691***	−1.097	3.492***	−5.349***	0.620		(0.000)
EBITDA	(n = 1,867)								
Coefficient	*t*+1	0.200	0.597	−1.273	0.441	−0.008	−0.002	0.375	124.48
White t-stat		5.663***	7.405***	−1.978**	1.940	−3.854***	−1.601		(0.000)
Coefficient	*t*+2	0.333	0.574	−0.282	0.883	−0.016	−0.002	0.260	73.72
White t-stat		8.298***	7.006***	−0.414	2.568***	−6.463***	−1.227		(0.000)
Coefficient	*t*+3	0.449	0.594	−0.137	1.482	−0.025	−0.002	0.207	55.05
White t-stat		9.014***	5.568***	−0.151	3.710***	−7.972***	−0.959		(0.000)
OPINC	(n = 1,595)								
Coefficient	*t*+1	0.135	0.51	−1.242	0.281	−0.005	−0.001	0.230	53.89
White t-stat		4.368***	5.703***	−2.653***	1.103	−1.996**	−1.206		(0.000)
Coefficient	*t*+2	0.242	0.45	−0.134	0.646	−0.012	−0.002	0.140	30.02
White t-stat		6.801***	5.427***	−0.286	1.674	−4.348***	−0.894		(0.000)
Coefficient	*t*+3	0.343	0.343	0.390	1.019	−0.020	−0.002	0.101	19.80
White t-stat		9.011***	4.213***	0.553	2.822***	−6.240***	−0.248		(0.000)

(*continued on next page*)

TABLE 6 (continued)

***, ** denote significance at the 0.01 and 0.05 levels, respectively.

[a] Fiscal-year fixed-effects indicator variables are included in the equation but not presented. Firm subscripts are omitted.

Variable definitions:

Dependent variable *PERF* = one of three measures of performance namely cash flow from operations (*CFO*), earnings before interest, taxes, depreciation and amortization (*EBITDA*), or operating income (*OPINC*), n years in the future where n = 1, 2, 3;
ΔRDT = change in recognized net deferred taxes from year t to year $t-1$;
ΔUDT = change in unrecognized net deferred taxes from year t to year $t-1$;
SIZE = the natural logarithm of market value; and
MTB = the market-to-book ratio at time t.

PERF, ΔRDT, and ΔUDT are divided by beginning market value.

possibility to influence our tests; not only do we find strong results for all three profitability measures, we also measure our profitability variables *before* deferred taxes. So, if management manipulates the future profitability measures, then it would have to do so through accounting items other than deferred taxes.

VII. CONCLUDING REMARKS

We examine whether U.K. managers use the flexibility provided under the partial method for deferred taxes to measure unrecognized deferred taxes opportunistically. Overall, our results suggest U.K. managers measure deferred taxes opportunistically to manage the balance sheet but not the income statement. Our first tests examine whether firm-specific operational and opportunistic factors are associated with the level of unrecognized deferred taxes, and provide evidence certain U.K. managers opportunistically measure deferred taxes to manage leverage. Our findings confirm arguments by commentators that deferred taxes heavily influence leverage indicators that play a prominent role in the U.K. contracting framework. Because the proper identification and measurement of both operational and opportunistic determinants of unrecognized deferred taxes influence our first tests, we complement them with a second set of tests investigating whether unrecognized taxes relate to future deferred tax reversals and future operating profitability of the firm. The tests show the components of deferred taxes predict both future deferred tax reversals and indicators of future profitability of the firm as predicted; they also show no evidence of opportunism.

By finding evidence of balance sheet management by U.K. managers, our results support the view of opponents of the partial provision method who have argued the method is too flexible and too subjective. However, the balance sheet management does not reduce the ability of unrecognized deferred taxes to predict future profitability. One immediate extension of our research would be to assess whether the market sees through the balance sheet management and/or is willing to accept the balance sheet management because the benefits of managing leverage outweigh the increased cost of capital associated with higher reported leverage. Another interesting avenue for future research would be to investigate whether companies experience real economic consequences such as increases in the cost of capital upon adoption of the full method of accounting for deferred taxes in the U.K.

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Analysts' Forecasts and Brokerage-Firm Trading

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ABSTRACT: Using unique data on brokerage-firm trading, I examine whether analysts' earnings forecasts and stock recommendations affect their brokerage firms' share of trading in the forecast stocks. I find that individual analyst's forecasts that differ from the consensus forecast generate significant brokerage-firm trading in the forecast stocks in the two weeks after the forecast release date, affirming that analysts' forecasts affect their brokers' commission revenue. However, I find no evidence that analysts' forecast errors—the difference between forecast earnings and actual earnings—increase brokerage-firm trading. This result suggests that analysts cannot generate trade for their employers simply by adding error to their forecasts. I find that buy recommendations generate relatively more trading, both buying and selling, through the analyst's brokerage firm. Collectively, these results suggest that analysts can generate higher trading commissions through their positive stock recommendations than by biasing their forecasts.

Keywords: *earnings forecasts; trading incentives; brokerage-firm trading.*

Data Availability: *The data are available from I/B/E/S and the Toronto Stock Exchange.*

I. INTRODUCTION

Sell-side research analysts must maintain good relations with company management, assist the investment bank's underwriting department in marketing stock offerings, and serve the institutional clients who provide commission revenue to their brokers. Incentives to meet these goals could potentially bias sell-side analysts' earnings forecasts and recommendations. Recently, concern that brokerage-firms' sell-side analysts issue biased research has driven lawsuits against Credit Suisse First Boston (Craig 2002), Merrill Lynch (Beck 2002), and Salomon Smith Barney (Silverman 2002). The resolution of these

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lawsuits could fundamentally change analysts' research.[1] Much of this controversy centers on whether underwriting relationships bias analysts' research. This paper investigates the largely unexplored question of how incentives to generate brokerage commissions for their employers—hereafter termed *trading incentives*—could affect analysts' earnings forecasts and stock recommendations.

Trading incentives are important because every forecast or recommendation can potentially generate trade for the analyst's employer. For example, Konrad (1989, 118) reports that a sell-side analyst at Morgan Keegan earned 2.5 percent of the brokerage's trading commissions in the 19 stocks the analyst covered. Dorfman (1991) also reports that some brokerage firms include similar trading incentives in analysts' contracts. However, more often brokerage firms conduct a formal poll asking the institutional sales force to rate analysts on how much trade they generate, and the results affect analysts' bonuses (Dorfman 1991; Laderman 1998; Irvine 2000; Lauricella 2001).

I examine whether analysts' forecasts and recommendations influence the amount of trading done by their employers in the forecast stock. I use Toronto Stock Exchange (TSE) data that identifies the broker(s) involved in every trade to calculate brokerage-firm market share of trading in the forecast stock. I apply Hayes' (1998) theoretical model of trading incentives to derive testable hypotheses about the relation between analysts' forecasts and their brokerage-firms' market shares in the forecast stocks. I then test whether, after controlling for analysts' recommendations, analysts' forecasts significantly affect brokerage-firm market share when the forecast is released.

As predicted, I find that the difference between an individual analyst's forecast and the consensus forecast significantly increases a broker's market share of trading in the forecast stock in the two weeks after I/B/E/S receives the analyst's forecast. This conclusion holds after controlling for the issuing analyst's contemporary recommendation on the stock. These findings suggest the possibility that trading incentives encourage analysts to issue forecasts further from the consensus to increase their commission-related compensation. To test whether trading incentives cause analysts to bias their earnings forecasts, I examine whether forecast errors, defined as the difference between an analyst's earnings forecast and actual earnings as reported by I/B/E/S, generate trading in the forecast stock for the analyst's brokerage firm. I find that forecast errors do not increase brokerage-firm market share, so adding error to their forecasts is not an effective way for analysts to generate trade.

Analysts' stock recommendations that accompany earnings forecasts can significantly increase brokerage-firm trading in the forecast stock in the two weeks after I/B/E/S receives the forecast. I find that analysts' buy and strong buy recommendations allow their brokerage-firms' to capture significantly higher market share of trading—both buying and selling—than do hold, sell, or strong sell recommendations. Because buy and strong buy recommendations generate trade effectively, trading incentives could lead analysts to skew their recommendations toward buy and strong buy recommendations. Thus, my findings suggest that, as long as investment bank research is paid for with trading commissions, the potential for biased recommendations remains, even if regulators remove analysts' incentives to promote their firms stock offerings by effectively separating research departments from underwriting departments (Kahn 2002).

Focusing on how analysts' forecasts and recommendations generate trading for their brokerage firms enables me to extend empirical research that directly links analysts' compensation incentives to their forecasts and recommendations. Recent research finds that the probability an analyst will leave her current position increases if her forecasts are less accurate than those of her peers (Mikhail et al. 1999), and that analysts adjust their earnings forecasts in response to underwriting compensation incentives (Dechow et al. 2000). Dechow et al. (2000) find that analysts issue over-optimistic long-term earnings forecasts for clients of their firm's underwriting department when those clients issue common stock. They report that the optimism in analysts' long-term forecasts is positively related to the amount of fee revenue the stock offering generates for the analysts' brokerage firm, an important component of analysts' annual bonuses at many brokers (Lowenstein 1996).

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[1] Proposed solutions include splitting research into organizations that are independent from brokerage-firms' underwriting departments (Kahn 2002), or creating a research oversight board (Gasparino and Smith 2002).

My results are consistent with the theoretical prediction that the incentive to generate trading commissions for their brokerages can be an important influence on analysts' decisions (McNichols and O'Brien 1997). My results are also consistent with Hayes' (1998) prediction that analysts' forecasts affect their brokerage-firms' commission revenues. However, other empirical results are inconsistent with existing theory. Thus, my study also provides a set of empirical results that can guide refinements of theory on how trading incentives affect analysts' forecasts and recommendations.

The paper proceeds as follows. Section II develops hypotheses about how analysts' earnings forecasts affect brokerage-firm trading. Section III describes the sample and key variables used in the empirical tests. Section IV examines the empirical relation between brokerage-firm market share of trading and analysts' forecasts and recommendations. Section V concludes.

II. HYPOTHESES

This section applies existing theory to develop hypotheses that predict how investors' trading demands respond to analysts' earnings forecasts. Admati and Pfleiderer (1990), Allen (1990), Brennan and Chordia (1993), and Hayes (1998) all model how utility-maximizing investors respond to a signal provided by an information seller, such as a sell-side analyst. I focus on Hayes' (1998) model because it explicitly analyzes investors' trading demands using analysts' earnings forecasts as the informative signal.

The Hayes (1998) Model

Hayes (1998) presents a partial equilibrium model of risk-averse investors with negative exponential utility who must allocate their wealth, Wt, between a riskless asset and a risky asset (stock). The analyst provides investors with information about the stock's expected return. The price of the riskless asset and its terminal payoff are normalized to 1. The risky asset's price is P, and its expected terminal payoff is $P + x$, where x is the commonly known expected return, and $x > 0$. The asset's actual terminal payoff is $P + x + \mu$. In her model, investors and the analyst share common prior beliefs about μ, namely that it is distributed normally with mean zero and variance 1. At the beginning of the period, investors own m shares of stock.

Investors allocate their wealth based on the information the analyst provides. Hayes (1998) models the analyst as issuing a report consisting of two components: her posterior expectation of μ, denoted by μ_R, and the variance of this estimate, σ_R. The posterior expectation, μ_R, can be viewed as the analyst's privately held signal about the accuracy of the consensus earnings forecast since, in Hayes' model, the stock's expected return (x) is common knowledge and equals the expected economic earnings for the period. The investor pays a commission, c, to the broker for each share of stock bought and sold.

As Hayes (1998, 302) demonstrates, an investor chooses the number of shares demanded, n, to maximize his certainty equivalent wealth given the information in the analyst's forecast:

$$(W - nP) + n(P + x + \mu_R) - \frac{\rho}{2} n^2\sigma_R^2 - c|m - n| \tag{1}$$

where ρ is the coefficient of risk aversion. Hayes (1998) shows that the solution to this problem results in two demand equations. The first equation defines the number of shares bought, n_b, if the analyst's signal is favorable, ($\mu_R > 0$):

$$n_b = \frac{x + \mu_R - c}{\rho\sigma_R^2}. \tag{2}$$

Alternatively, if the information in the analyst's report is unfavorable ($\mu_R < 0$), the number of shares sold, n_s, is given by:

$$n_s = \frac{x + \mu_R + c}{\rho\sigma_R^2}. \tag{3}$$

If trading were costless ($c = 0$), then analysis of investors' demand with respect to the information in the forecast is straightforward. The more positive an analyst's earnings forecast, conditional on the forecast exceeding the consensus earnings expectation, x, (i.e. $\mu_R > 0$), the more investors wish to purchase. The more negative an analyst's earnings forecast, conditional on the forecast falling below the consensus earnings expectation, x, (i.e., $\mu_R < 0$), the more investors wish to sell. Together these results generate the first empirical hypothesis from Hayes' (1998) model:

H1: The greater the absolute deviation in $|\mu_R|$, the greater brokerage-firm trading in the forecast stock.

Differentiating Equations (2) and (3) with respect to σ_R^2, the variance of the analysts' expectation of the consensus forecast error (μ_R), shows that the number of shares investors wish to buy and sell is decreasing in σ_R^2. In Hayes' (1988) model, σ_R^2 is the only source of uncertainty; investors, however, are likely to face additional sources of uncertainty. In particular, uncertainty surrounding the accuracy of the consensus forecast. I define total uncertainty surrounding the earnings forecast to include both uncertainty surrounding the analyst's expectation of the consensus forecast error and uncertainty surrounding the accuracy of the consensus, and predict that:

H2: Brokerage-firm trading in the forecast stock decreases in the total uncertainty of the forecast.

Hypothesis 2 is consistent with the common theoretical prediction that the extent to which investors trade on information decreases as the uncertainty of that information increases.[2]

Trading costs complicate the analysis. Figure 1 adapts Hayes' (1998, 303) diagram of the trading demands derived in Equations (2) and (3). The figure presents the investors' trading demands n, (i.e., the difference between the investor's optimal holdings and his

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[2] Admati and Pfleiderer (1990), Allen (1990), and Brennan and Chordia (1993) all examine how investors respond to uncertain private information. Kim and Verrecchia (1991) extend their results to the case of uncertainty in both private and public information.

FIGURE 1
Investor Trading Demand as a Function of an Analyst's Forecast

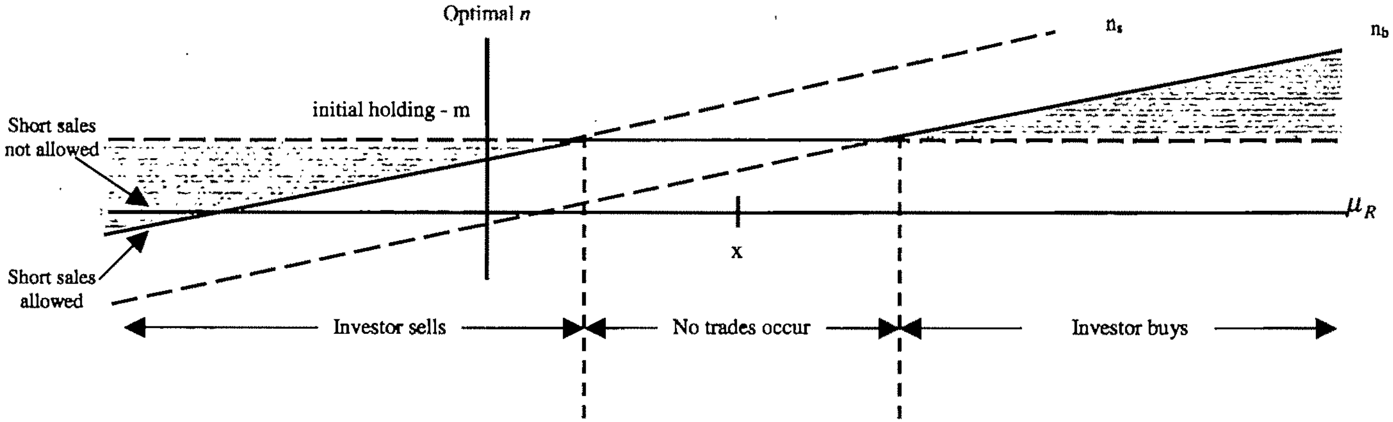

Figure 1, adapted from Hayes (1998, 303). This diagram shows an investor's optimal number of shares, n, as a function of the deviation in an analyst's forecast from the consensus forecast x. m denotes the investor's initial holdings given the consensus forecast, x. n_b, the number of shares the investor buys, is the solution to Equation (2) given that, μ_R, the analyst's estimate of the error in the consensus forecast, is greater than zero. The solid portion of line nb represents values of μ_R for which n_b is positive and the investor purchases shares. The dashed portion of line n_b represents values of μ_R for which n_b is negative and the investor does not purchase shares. ns, the number of shares sold, is the solution to Equation (3) given μ_R is less than zero. The solid portion of line n_s represents values of μ_R for which ns is positive and the investor sells shares. The dashed portion of line ns represents values of μ_R for which ns is negative and the investor does not sell shares. Note that: (1) the number of shares traded is not symmetric in $|\mu_R|$ if short selling is constrained, and (2) no trades occur when $|\mu_R| <$ the commission charge, c.

initial holdings, m). Conditional on x, the consensus forecast, investors initially hold m shares. For small deviations between the analyst's forecast and the consensus forecast, $|\mu_R| < c$, a region of no trading exists. When μ_R is large and negative, investors' optimal position in the risky asset falls below zero, and investors sell short. Hayes (1998), following Diamond and Verrecchia (1987), incorporates the fact that some investors, such as mutual funds, are restricted from short selling.[3] Restrictions on short selling produces an asymmetry in investors' trading demands between negative and positive values of μ_R.[4]

> **H3:** A positive μ_R generates more brokerage-firm trading than a negative μ_R of the same magnitude.

Hayes (1998), therefore, argues that the marginal return from analysts' efforts in gathering forecast information is greater for stocks that the analyst expects to perform relatively well. As a consequence, she predicts forecast accuracy will be greater for stocks that the analyst expects to perform well in the future. A test of H3 provides evidence on whether asymmetric trading incentives are likely to cause this behavior.

Operationalizing the Theoretical Constructs

To test these hypotheses, I must identify empirical proxies for $|\mu_R|$ and total uncertainty. My proxy for $|\mu_R|$ is the absolute deviation between an analyst's earnings forecast and the consensus earnings forecast. The consensus forecast is the I/B/E/S-reported consensus earnings forecast in the month prior to the analyst's forecast. To scale forecast deviations across firms, I deflate the absolute deviation between the forecast and consensus by the stock price on the day of the forecast. Following H1, I predict that the greater the price-deflated absolute deviation between the analyst's earnings forecast and the consensus forecast (*ABSDEV*), the more likely that investors' valuations will change enough to outweigh the transaction costs of trading. If H1 holds, then I expect *ABSDEV* to be positively related to brokerage-firm trading in the forecast stock.

To calculate an empirical proxy for total uncertainty associated with an analyst's earnings forecast, I use the Barron et al. (1998) (hereafter BKLS) measure of uncertainty, which defines uncertainty over both common (in the consensus) and idiosyncratic (the dispersion across all analysts' forecasts) information:

$$UNCERTAINTY = \left(1 - \frac{1}{N}\right) D + SE \tag{4}$$

where N is the number of forecasts, and D is the sample variance of analysts' forecasts:

$$D = \frac{1}{N-1} \sum_{a=1}^{N} (F_a - \bar{F})^2. \tag{5}$$

and SE is the sample squared error in the consensus forecast:

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[3] Even when not completely restricted, investors do not have full use of the proceeds from the short sale, making short sales relatively costly.

[4] I thank the referee for this comment.

$$SE = (A - \bar{F})^2. \tag{6}$$

In these equations, A is the actual earnings realization, F_a is the forecast by analyst a, and $\bar{F}$ is the consensus forecast.

When making their trading decisions, investors must consider both the analyst's forecast dispersion and the possibility of error in the consensus forecast. BKLS's *UNCERTAINTY* includes proxies for uncertainty in analyst-specific information, (Hayes' [1998] σ_R), through the forecast dispersion component, D, and uncertainty surrounding the accuracy of the consensus forecast through *SE*. Under H2, if *UNCERTAINTY* captures investors' total uncertainty surrounding the analyst's forecast, then *UNCERTAINTY* will be negatively related to brokerage-firm trading in the forecast stock.

My hypotheses predict that analysts' forecasts affect their brokerage-firm's trade. Hayes (1998, 304) assumes that analysts, through trading commissions, capture the benefits of releasing their forecasts. However, McNichols (1990) correctly maintains that investors are not contractually obligated to trade through the broker from whom they receive the report. Irvine (2000) reports that the market for analysts' research in both the U.S. and Canada is based on "soft-dollar" payments. Instead of paying cash for research services, institutional investors pay through commissions on their trading activity. Irvine (2000) shows that, as a consequence of the "soft-dollar" market, analyst coverage of a particular stock is associated with higher brokerage-firm market share in the covered stock. This result suggests a link between analysts' activities and brokerage-firm trading, but does not determine whether particular analysts' forecasts and recommendations exert a direct affect on brokerage-firm trading. One contribution of the current study is to provide empirical evidence on whether brokers capture *any* incremental commission payments when their analysts' release forecasts and recommendations.

Extensions of the Hayes (1998) Framework

Analysts commonly release investment recommendations along with their earnings forecasts. Substituting analysts' recommendations for the information in analysts' forecasts within Hayes' (1998) framework suggests that positive (buy) and negative (sell) recommendations would generate more brokerage-firm trading in the recommended stock than neutral (hold) recommendations.[5] If analysts' recommendations subsume the information in their earnings forecasts, then after controlling for the recommendation, neither *ABSDEV* nor *UNCERTAINTY* would predict brokerage-firm trading in the forecast stock. However, Francis and Soffer (1997) find that analysts' earnings forecasts and stock recommendations contain distinct price-relevant information. If analysts' forecasts and stock recommendations both contain distinct price-relevant information, then both could influence brokerage-firm trading in the forecast stock. In Section IV, I examine this issue and find that the deviation between the forecast and consensus consistently predicts brokerage-firm market share in the forecast stock, even after controlling for the analyst's recommendation.

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[5] Practitioners sometimes maintain that hold recommendations are *de facto* sell recommendations. In fact, I show below that no significant difference in brokerage-firm market share exists between hold and sell recommendations.

This result is a natural extension of Francis and Soffer (1997) since investors should consider the price-relevant information in both earnings forecasts and recommendations when making their trading decisions.[6]

Finally, I examine whether analysts' forecast errors, defined as the difference between an analyst's forecast and I/B/E/S reported actual earnings, increase brokerage-firm trading in the forecast stock. If *ABSDEV*, the distance of an analysts' forecast from the consensus forecast, increases brokerage-firm trading, then instead of reporting her true forecast, an analyst could deliberately add error to her forecast in order to increase *ABSDEV* and, therefore, her commission-related compensation. Consider an analyst whose true unbiased forecast of upcoming annual earnings is 10 cents per share, higher than the consensus forecast of 9¢. If the distance of her forecast from the consensus forecast generates trading-commission revenue for her employer, then she could increase her firm's commission revenue by sacrificing forecast accuracy and reporting a forecast further from the consensus, say 11¢. I cannot observe her actual *ex ante* forecast error, but I can observe *ex post* forecast errors—the difference between the analyst's forecast and actual earnings. Thus, I test whether *ex post* forecast errors generate trading flow for the analyst's brokerage firm.

III. DATA

The Toronto Stock Exchange Data Set

This paper uses September 1, 1993 through August 31, 1994 transaction data from the Toronto Stock Exchange (TSE), the largest stock exchange in Canada and the seventh largest in the world. The average daily turnover in 1993 was 59.0 million shares, representing a value of C$583 million. These measures of average trading grew in 1994, when daily trading averaged 61.5 million shares, or C$726 million. Trading on the TSE occurs in a market-maker system. Every trade occurs through a seat holder on the exchange, a broker who can trade as an agent or as a principal. The advantage of TSE data is that when the TSE documents a trade, it records, in addition to time, volume, and price information, two brokerage identification codes. The TSE assigns each seat holder a unique two-digit code, which identifies the brokerage firm that sold the security and the brokerage firm that bought the security for each trade.

Irvine (2000) argues that because of the similarities between the sell-side analyst environments in Canada and the U.S., conclusions based on Canadian data are likely to apply to the U.S. market. Specifically, Irvine (2000) reports that: (1) institutional investors are significant traders in both markets, (2) the institutional practice of directing soft-dollar commissions to brokers in return for analyst research enjoys the same safe harbor provisions under the Ontario Securities Act as exist under Section 28(e) of the U.S. Securities Exchange Act of 1934, and (3) that the amount of commission revenue analysts generate for their employers forms an important component of Canadian analysts' compensation.

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[6] Reiterations of previous forecasts or the existence of sophisticated investors do not invalidate the predictions derived from Hayes (1998), but they could reduce the economic importance of the model's predictions. Hayes (1998) assumes that prior to receiving the analyst's earnings forecast, the market's valuation is based entirely on the consensus forecast. There is no mention of reiterations in this framework, but if the analyst's forecast reiterates an earlier forecast, the forecast can generate trade if the reiteration reduces investors' uncertainty about the accuracy of the analyst's forecast. Sophisticated investors, aware of the analyst's trading incentives, could discount forecasts far from the consensus as attempts to generate trade. However, if trading incentives do not completely dominate analysts' incentives to produce accurate forecasts, sophisticated investors would place some weight on the analyst's forecast. Given the evidence in Mikhail et al. (1999) that inaccuracy relative to their peers increases the probability of turnover, it is reasonable to assume that analysts are concerned about the accuracy of their forecasts.

I assign trading volume to the brokerage-firm(s) whose identification code accompanies a trade record.[7] I then summarize brokerage-firm volume by calculating each firm's market share. Market share is the number of shares traded through the brokerage firm normalized by total shares traded in the stock. Specifically, the daily market share of brokerage firm *j* for stock *k* is the total volume traded by brokerage firm *j* in stock *k* on day *i*, divided by the total volume in stock *k* on day *i*. In Equation (7), the sum of brokerage-firm volume for all *J* brokers on the TSE represents total volume for stock *k*:

$$MKT\_SHARE^{k}_{j,i} = \frac{Broker\ Volume^{k}_{j,i}}{\sum_{j=1}^{j} Broker\ Volume^{k}_{j,i}}. \tag{7}$$

I also calculate buy-side and sell-side *MKT_SHARE* using only buy or sell trades, respectively. For example, a brokerage-firm's buy-side *MKT_SHARE* represents the percentage of all purchases that occur in a particular stock-day executed by that brokerage firm. In untabulated tests, I find that *MKT_SHARE* is significantly positively correlated with the number of shares traded by the brokerage firm ($\rho = 0.71$). *MKT_SHARE* is, therefore, strongly related to commission-based trading revenue, which is most often charged on a cents-per-share basis (Conrad et al. 2001). *MKT_SHARE* has other desirable properties as a measure of brokerage-firm trading: *MKT_SHARE*, unlike raw brokerage-firm volume, is not autocorrelated, and it is also uncorrelated with total trading in the stock; so exogenous changes in total stock volume cannot bias the empirical tests. Summing daily *MKT_SHARE* over any period *T* produces the total broker *MKT_SHARE* over that period.

The Sample

The TSE sample includes every trade of the largest 100 companies on the TSE between September 1, 1993 and August 31, 1994. These 100 companies represent a significant fraction of the market value of all Canadian-based public companies. Ninety-seven of the 100 companies in the sample are in the TSE 300, the TSE's primary index. These 97 companies make up 78.2 percent of the value of the TSE 300 and include 18 of the 20 most active issues on the exchange (Toronto Stock Exchange 1994).

I obtain analysts' earnings forecasts and recommendations for the sample firms from the I/B/E/S International earnings forecast and recommendation databases. Over the sample period, the I/B/E/S detailed forecast database records a total of 1,224 forecasts of upcoming annual earnings (fiscal year one forecasts) whose issuing brokers also have recommendations on the I/B/E/S detailed recommendations tape. I delete 264 forecasts because the issuing broker was not a member of the Toronto Stock Exchange on July 31, 1993.[8] In addition, to ensure that earnings announcements do not confound the results, I delete 125 earnings forecasts issued within five trading days before or after an earnings announcement.[9] The final I/B/E/S sample consists of 835 analysts' earnings forecasts issued through 15 different brokerage firms on 96 of the 100 largest stocks on the TSE.

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[7] The TSE allows brokers to internalize (same broker buying and selling) trades. Therefore, some trades involve two brokers and others only one.

[8] Most of the deleted forecasts were issued by U.S.-based brokers or brokers whose firm name in the 1993 TSE record could not be conclusively matched to a name in the I/B/E/S historical names file. In addition, I delete 33 forecasts made by the broker HSBC Securities, which is currently a member of the TSE, but was not a member firm according to TSE records on July 31, 1993 (Toronto Stock Exchange 1993).

[9] The various news services reporting to Bloomberg supply the earnings announcement dates.

Table 1 presents descriptive statistics on brokerage trading and coverage activity for the 15 I/B/E/S sample brokerage firms in the 100 largest TSE stocks. The 15 brokerage firms constitute a diverse group that trade 54.8 percent of total *MKT_SHARE* in these stocks.[10] The table presents mean daily brokerage-firm volume (C$) and *MKT_SHARE* per stock, the total number of sell-side analysts each brokerage firm employs (Number of Analysts), the number of sample stocks each brokerage firm covers (Number of Stocks Covered), and the number of sample forecasts (Number of Forecasts in Sample).[11] Brokerage firms with larger *MKT_SHARE* tend to cover more stocks. As a consequence, these brokers contribute more forecasts to the sample. All else equal, smaller firms will have less

TABLE 1

Brokerage Trading and Coverage in the 100 Largest Stocks Traded on the Toronto Stock Exchange

Broker	**Mean Brokerage $ Volume**	**Mean Brokerage-Firm Market Share**	**Number of Analysts**	**Number of Stocks Covered**	**Number of Forecasts in Sample**
Barclay De Zoete Wedd	53,767	1.45	9	58	75
Bunting Warburg	107,653	1.96	13	66	2
Burns Fry	538,074	9.64	22	97	160
First Marathon	511,725	6.43	6	41	67
Goepel Shields	25,471	0.46	6	32	9
Levesque Beaubien	111,949	3.61	11	63	145
Loewen Ondaatje	94,882	1.70	7	37	52
Majendie	61,268	1.39	5	40	1
Marleau Lemire	30,385	0.68	13	1	2
Midland Walwyn Capital	270,449	6.00	12	57	45
Research Capital	23,335	0.67	6	43	32
Richardson Greenshields	311,801	5.48	14	81	155
Sanwa McCarthy	18,089	0.49	9	35	18
ScotiaMcLeod	401,223	6.17	15	77	10
Wood Gundy	503,131	8.62	13	82	62

Mean Brokerage $ Volume = the average daily C$ share volume per stock traded by the brokerage;

Mean Brokerage-Firm Market Share = the brokerage's average daily *MKT_SHARE* per stock for the 100 largest stocks trades on the Toronto Stock Exchange. The daily *MKT_SHARE* of brokerage firm *j* in stock *k* is the total volume traded by brokerage firm *j* in stock *k* on day *i*, divided by the total volume in stock *k* on day *i*;

Number of Analysts = the total number of sell-side analysts the brokerage firm employs;

Number of Stocks Covered = the number of stocks in the sample the brokerage-firm covers. I determine stock coverage from information the brokerages provide to *Nelson's Directory of Investment Research* (1994); and

Number of Forecasts in Sample = the number of distinct earnings forecasts and recommendations pairs reported in the I/B/E/S International detailed earnings forecast and recommendation databases.

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[10] Discount brokers, institutional dealers, and full-service brokers who did not report new forecasts to I/B/E/S during the sample period execute the remaining trades.

[11] Stock coverage information comes from the 1994 *Nelson's Directory of Investment Research*, International Edition.

commission revenue available to support a research department. One brokerage firm covers just one sample stock, concentrating instead on small- and mid-cap stocks.

Event Windows

The statistical tests examine four event windows surrounding the release of the forecast:

1. Days −10 through −6 relative to the forecast date
2. Days −5 through −1 relative to the forecast date
3. Days 0 through +5 relative to the forecast date
4. Days +6 through +10 relative to the forecast date.

Discussions with analysts and research directors suggest that a two-week trading window likely captures the immediate effect of analysts' forecasts on investors' trading decisions. I examine period 2, days −5 through −1 relative to the forecast date, because analysts' forecasts could stimulate trading flow through their brokerages in this period if analysts release their forecasts to their best customers before they release them to I/B/E/S. Period 1, days −10 through −6, is a control period when no relation between *ABSDEV* and brokerage-firm trading should exist. Brokerage-firm market share should not be related to analysts' forecasts until the forecast has been prepared and released.

Key Independent Variables

Table 2 presents summary statistics for the 835 analysts' forecasts in the I/B/E/S sample. The price-deflated absolute deviation between the forecast and consensus (*ABSDEV*), and the BKLS measure of uncertainty (*UNCERTAINTY*) are defined above. Mean Market Share, which measures *MKT_SHARE* over all 252 trading days for the unique

TABLE 2
Summary Statistics for 835 I/B/E/S Analysts' Forecasts on 96 Sample Stocks

Variable	Mean	Median	Skewness	Standard Deviation
MARKET SHARE	5.71%	4.31%	1.59	4.74%
ABSDEV	0.69%	0.29%	3.15	1.08%
UNCERTAINTY	0.18	0.04	3.82	0.40
COVERAGE	11.86	11	1.29	5.45
SIZE	2,766	1,599	2.24	2,894
PRICE	25.53	21.97	1.60	13.94

MARKET SHARE = Market Share is $MKT\_SHARE^{*}_{ij}$ from Equation (7) averaged first over time i for each of the 428 unique brokerage firm-stock $\{j, k\}$ pairs in the sample, then across the brokerage firm-stock pairs;
ABSDEV = the absolute value of the difference between the analyst's fiscal year one earnings forecast and the consensus forecast all divided by the stock price on the forecast date;
UNCERTAINTY = the Barron et al. (1998) measure of uncertainty $= \left(1 - \frac{1}{N}\right) D + SE$, in C$ per share squared. N is the number of forecasts, D is the sample variance of analyst's forecasts, and SE is the sample squared error in the consensus forecast;
COVERAGE = the total number of fiscal year one analysts' forecasts reported to I/B/E/S in the month preceding the each stock's initial sample forecast;
SIZE = year-end 1993 market value of the 96 stocks in the sample, in C$ millions; and
PRICE = a stock's initial forecast date stock price in C$.

brokerage firm-stock combinations in the sample is 5.71 percent.[12] Mean Market Share reflects the average trading activity of the sample brokerage firms in the forecast stocks. Mean *ABSDEV* is 0.69 percent. Untabulated analyses reveal that the mean (median) absolute difference between the fiscal year one forecast and the consensus forecast is C\$0.14 (\$0.07). Four hundred thirty forecasts are above the consensus forecast, 374 are below, and 31 are equal to the consensus. Mean *UNCERTAINTY* is C\$0.18 per share squared, though the median is much lower (0.04).[13] The average analyst coverage per stock (*COVERAGE*) is just under 12, the average stock market capitalization per stock (*SIZE*) is just under C\$2.8 billion, and the average share price is C\$25.53. *ABSDEV* is significantly correlated with *UNCERTAINTY* ($\rho = 0.422$), which reflects the fact that forecast dispersion is a component of the BKLS uncertainty measure. This significant correlation between two regressors indicates the need for collinearity diagnostic tests on the regression results.

IV. EMPIRICAL TESTS

Using data from each of the event windows described above, Table 3 presents the results of estimating the following regression:

$$SUMSHARE = \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \sum_{j=3}^{7} \beta_j INDDUMMY_j + \varepsilon_i. \qquad (8)$$

The dependent variable, *SUMSHARE*, is the sum of daily broker *MKT_SHARE* in the stock over the days in each event window. In addition to *ABSDEV* and *UNCERTAINTY*, Equation (8) includes five broad industry classification dummy variables first used by Bhushan (1989a, 1989b): *MINING* (two-digit SIC codes 10–14), *MANUFACTURE* (two-digit SIC codes 15–39), *UTILITY* (two-digit SIC codes 40–49), *TRADE* (two-digit SIC codes 50–59), and *FINANCIAL* (two digit SIC codes 60–67). The impact of a sixth group, the *SERVICES* industry (two-digit SIC codes 70–96), emerges implicitly in the intercept term. These variables effectively control for heteroscedasticity related to the fact that utilities tend to have higher average *SUMSHARE*s. When I include the industry dummy variables in the regression, the results fail to reject the null hypothesis of no heteroscedasticity using White's (1980) test.[14]

If the deviation between an analyst's earnings forecast and the consensus forecast affects investors' trading demands, then H1 predicts that the coefficient on *ABSDEV* should be positive. Hypothesis 2 predicts that investors' trading demands are inversely related to the total uncertainty of the forecast. Thus, I expect the coefficient on *UNCERTAINTY* to be negative.

Analysts' Forecasts and their Brokerage's Share of Trading: Testing H1 and H2

Table 3 presents OLS estimates of Equation (8).[15] Overall, the results suggest that analysts' earnings forecasts can significantly increase their brokerage-firm's share of trading

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[12] Equation (7) shows how to calculate $MKT\_SHARE^{k}_{ij}$ for each day i, brokerage firm j, and stock k. Mean Market Share is $MKT\_SHARE^{k}_{ij}$ averaged first over time for each of the 428 unique brokerage firm-stock $\{j, k\}$ pairs in the sample, then across the brokerage firm-stock pairs.

[13] The units of the square root of the BKLS *UNCERTAINTY* measure are cents per share.

[14] Excluding the industry dummy variables from the regression does not significantly affect any of the study's inferences.

[15] The sample size in the day +6 through +10 period is 829 because analysts issue six forecasts in the last week of the sample year. These forecasts' event days +6 through +10 lie outside the sample period.

TABLE 3
The Effects of the Deviation of Analysts' Earnings Forecasts from Consensus and Total Forecast Uncertainty on Brokerage Market Share

Regression:

$$SUMSHARE = \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \sum_{j=3}^{7} \beta_j INDDUMMY_j + \varepsilon_i.$$

	Dependent Variable	Dependent Variable	Dependent Variable	Dependent Variable
Independent Variable	**Broker Market Share in Event Days −10 through −6**	**Broker Market Share in Event Days −5 thorugh −1**	**Broker Market Share in Event Days 0 through +5**	**Broker Must Share in Event Days +6 through +10**
Intercept	0.23	0.28	0.32	0.32
	(5.78)	(7.01)	(6.56)	(7.78)
ABSDEV	1.19	2.41	4.28	3.14
	(0.96)	(1.97)	(2.68)	(2.42)
UNCERTAINTY	−0.03	−0.05	−0.07	−0.09
	(−0.89)	(−1.29)	(−1.73)	(−2.59)
Control Variables				
MINING	0.03	−0.04	−0.06	−0.07
	(0.64)	(−0.98)	(−1.05)	(−1.71)
MANUFACTURE	0.07	0.04	−0.02	−0.10
	(1.31)	(0.67)	(−0.24)	(−1.98)
UTILITY	0.17	0.07	0.18	0.06
	(2.65)	(1.03)	(2.21)	(0.84)
TRADE	0.02	−0.01	0.08	−0.05
	(0.39)	(−0.23)	(1.12)	(−0.78)
FINANCIAL	0.05	−0.01	0.04	−0.07
	(0.94)	(−0.10)	(0.53)	(−1.17)

(continued on next page)

TABLE 3 (continued)

R^2	1.22%	1.30%	2.65%	2.13%
Adjusted R^2	0.38%	0.48%	1.83%	1.31%
F-test p-value	0.11	0.22	0.01	0.11
n	835	835	835	829

t-statistics appear in parentheses. F-test p-values are for the joint test of whether the five industry dummy variable coefficients are equal to zero.

SUMSHARE = the sum total of daily brokerage-firm *MKT_SHARE* in the forecast stock in each event window;

ABSDEV = the absolute value of the difference between the analyst's fiscal year one earnings forecast and the consensus forecast, divided by the stock price on the forecast date;

UNCERTAINTY = the Barron et al. (1998) measure of uncertainty $\left(1 - \frac{1}{N}\right) D + SE$, in C$ per share squared. N is the number of forecasts, D is the sample variance of analysts' forecasts, and SE is the sample squared error in the consensus forecast; and

INDDUMMY = *MINING*, *MANUFACTURE*, *UTILITY*, *TRADE*, and *FINANCIAL* sector dummy variables proposed by Bhushan (1989a, 1989b). The impact of the *SERVICES* industry is implicitly in the intercept term.

in the forecast stock. The more an analyst's forecast differs from the consensus forecast, the greater the broker's market share in the two weeks after forecast publication. Specifically, the coefficient on *ABSDEV* is positive and significant at the 0.05 level in the day −5 through −1 period and the day +6 through +10 period and is significant at the 0.01 level in the day 0 through +5 period. Finding that the deviation between an analyst's forecast and the consensus forecast is associated with the brokerage's share of trading in the stock over days −5 through −1 (before the forecast is released) is consistent with Brown et al. (1991), who find that analysts' clients can only trade profitably if clients receive analysts' forecasts prior to public release. I estimate the economic impact of forecast deviations from consensus by multiplying the coefficient on *ABSDEV* by its mean value (0.0069) and dividing by the number of days in the period. For example, the 4.28 coefficient on *ABSDEV* in the day 0 through +5 period represents an increase of 0.5 percent per day in brokerage market share, approximately a 10 percent increase in daily brokerage market share.

The significance of *ABSDEV* in the day −5 through day −1 period is not robust to excluding day −1 from the regression. This finding suggests either that analysts leak the contents of their reports one day before public release, or that some I/B/E/S forecast dates are one day late.

The coefficient on *UNCERTAINTY* is negative and significant at the 0.10 level over the day 0 through +5 period and at the 0.01 level over the day +6 through +10 period.[16] The greater the Barron et al. (1998) measure of total uncertainty surrounding upcoming annual earnings, the less trade an analyst's forecast generates for her brokerage firm. Multiplying the coefficient on *UNCERTAINTY* over the day 0 to +5 period by the mean of *UNCERTAINTY* (0.18) and dividing by the number of days in the period shows that *UNCERTAINTY*, at its mean, decreases brokerage-firm market share by 0.2 percent per day. This result is consistent with the common theoretical prediction that magnitude of investors' trading is inversely associated with the level of uncertainty. Neither *ABSDEV* nor *UNCERTAINTY* is significant in the control period, days −10 to −6. These regression results are consistent with H1 and H2.[17] *ABSDEV* is associated with higher brokerage-firm market share, while *UNCERTAINTY* is associated with lower brokerage-firm market share in the two weeks after the forecast release date.[18]

Aggregate Effects and below Consensus Forecasts

Table 4 presents three regression results that extend our understanding of how analysts' forecasts affect brokerage-firm market share.

The first regression is a benchmark for the next two. The benchmark simply replicates the specification in Equation (8) using, as the dependent variable, the sum of a brokerage firm's daily *MKT_SHARE* in the forecast stock over event days 0 through +10. Consistent with Table 3, the coefficient on *ABSDEV* is significantly positive at the 0.01 level and the

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[16] To test whether the skewness in *UNCERTAINTY* affects the results. I estimate Equation (8) substituting ln*UNCERTAINTY* for *UNCERTAINTY*. Using ln*UNCERTAINTY* produces similar inferences.

[17] Examination of the regression residuals indicates that intrafirm variation is small relative to interfirm variation. This finding raises the concern that correlation through time in firm-specific variables could affect the significance levels of the OLS t-statistics. I examine several alternative regression specifications that control for firm-specific time-series correlation including: generalized least squares, generalized method of moments, and Froot's (1989) model. These alternatives all produce larger t-statistics for *ABSDEV* and *UNCERTAINTY* than the OLS specification. Thus, the reported OLS test statistics are conservative.

[18] Collinearity does not significantly affect these inferences. The largest condition index in the set of regressions is 11.0. The results are not unduly influenced by particular observations. Excluding observations with the ten highest Cook's D statistics does not significantly affect the inferences. An analysis of the DFFITS and DFBETA statistics (Belsley et al. 1980) confirms this finding.

TABLE 4
The Effects of Below-Consensus Forecasts and Analysts' Stock Recommendations on Their Brokerage's Market Share of Trading in the Forecast Stock

Column 1:

$$SUMSHARE = \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \sum_{j=3}^{7} \beta_j INDDUMMY_j + \varepsilon_i.$$

Column 2:

$$SUMSHARE = \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \beta_3 ABSDEV \times NEG + \sum_{j=4}^{8} \beta_j INDDUMMY_j + \varepsilon_i.$$

Column 3:

$$SUMSHARE = \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \beta_3 BUY + \beta_4 SELL + \sum_{j=5}^{9} \beta_j INDDUMMY_j + \varepsilon_i.$$

	Dependent Variable	**Dependent Variable**	**Dependent Variable**
Independent Variable	**Broker Market Share in Event Days 0 through +10**	**Broker Market Share in Event Days 0 through +10**	**Broker Market Share in Event Days 0 through +10**
Intercept	0.61 (5.60)	0.61 (5.59)	0.41 (3.59)
ABSDEV	7.92 (3.06)	8.67 (2.75)	6.38 (2.49)
UNCERTAIN	−0.17 (−2.45)	−0.17 (−2.39)	−0.13 (−1.98)
ABSDEV × *NEG*		−1.64 (−0.41)	
BUY			0.26 (4.60)
SELL			0.09 (−1.02)
Control Variables			
MINING	−0.14 (−1.60)	−0.14 (−1.59)	−0.08 (−0.92)

(continued on next page)

TABLE 4 (continued)

MANUFACTURE	0.11	−0.11	−0.11
	(−1.10)	(−1.11)	(−1.13)
UTILITY	0.26	0.26	0.30
	(1.90)	(1.89)	(2.25)
TRADE	0.04	−0.05	0.07
	(0.39)	(0.40)	(0.58)
FINANCIAL	−0.01	−0.01	−0.01
	(−0.09)	(−0.06)	(−0.10)
R^2	2.91%	2.93%	6.30%
Adjusted R^2	1.85	1.75%	5.05%
F-test p-value			0.001
n	835	835	835

t-statistics appear in parentheses. The F-test p-value is for the test that the *BUY* coefficient and the *SELL* coefficient are equal.

SUMSHARE = the sum total of daily brokerage-firm *MKT_SHARE* in the forecast stock in event days 0 through +10;

ABSDEV = the absolute value of the difference between the analyst's fiscal year one earnings forecast and the consensus forecast all divided by the stock price on the forecast date;

UNCERTAINTY = the Barron et al. (1998) measure of uncertainty = $\left(1 - \frac{1}{N}\right) D + SE$, in C$ per share squared. *N* is the number of forecasts, *D* is the sample variance of analysts' forecasts, and *SE* is the sample squared error in the consensus forecast;

ABSDEV × *NEG* = is equal to *ABSDEV* if the analyst's forecast is below the consensus, and 0 otherwise;

BUY = a dummy variable set to 1 if the analyst's recommendation is a buy or a strong buy;

SELL = a dummy variable set to 1 if the analyst's recommendation is a sell or a strong sell; and

INDDUMMY = *MINING*, *MANUFACTURE*, *UTILITY*, *TRADE*, and *FINANCIAL* sector dummy variables proposed by Bhushan (1989a, 1989b). The impact of the *SERVICES* industry is implicitly in the intercept term.

coefficient on *UNCERTAINTY* is significantly negative at the 0.05 level. Thus, the distance of the individual forecast from consensus, and Barron et al. (1998) uncertainty, are significant determinants of brokerage market share in the two weeks after I/B/E/S receives the forecast.[19]

The second column of Table 4 tests whether a positive difference between an analyst's forecast and the consensus generates more trading than a negative difference of the same magnitude. Equation (9a) includes the variable *ABSDEV* × *NEG*, which estimates the slope shift in *ABSDEV* if the analyst's forecast is below the consensus. Hypothesis 3 predicts that analysts' forecasts that are below the consensus forecast have less effect on trading because short-sale constraints prevent many investors from trading on the negative information in these forecasts. Thus, H3 suggests the coefficient on *ABSDEV* × *NEG* will be negative.

$$SUMSHARE = \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \beta_3 ABSDEV \times NEG + \sum_{j=4}^{8} \beta_j INDDUMMY_j + \varepsilon_i. \tag{9a}$$

The second column in Table 4, however, shows that the coefficient on *ABSDEV* × *NEG* is not statistically significant.[20] Thus, my results are inconsistent with H3, which predicts an asymmetry between the effects of forecasts above the consensus and forecasts below the consensus. This finding is inconsistent with Hayes' (1998) argument that analysts have weaker trading incentives to gather information on stocks they view negatively.

Analysts' Forecasts, Stock Recommendations, and Brokerage-Firm Market Share

Equation (9b) examines whether H1 and H2 hold after controlling for analysts' contemporaneous buy and sell recommendations.

$$SUMSHARE = \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \beta_3 BUY + \beta_4 SELL + \sum_{j=5}^{9} \beta_j INDDUMMY_j + \varepsilon_i. \tag{9b}$$

BUY is a dummy variable set to 1 if the analyst's recommendation is a buy or strong buy. *SELL* is a dummy variable set to 1 if the analyst's recommendation is a sell or strong sell. There are 490 buy or strong buy recommendations in the sample and 82 sell or strong sell recommendations.

The third column in Table 4 presents the results of estimating Equation (9b). H1 and H2 still hold after controlling for analysts' stock recommendations. The coefficient on *ABSDEV* is positive (p-value < 0.01), and the coefficient on *UNCERTAINTY* is negative (p-value < 0.05). Given that the coefficient on *ABSDEV* is 6.38, at the mean (0.0069) *ABSDEV* increases market share by 0.4 percent per day.

Analysts' stock recommendations also produce a significant impact on brokerage-firm trading in the forecast stock. The 0.26 coefficient on *BUY* is significant at the 0.01 level and indicates that, over the day 0 through +10 period, buy recommendations generate approximately 2.4 percent per day higher market share in the recommended stock than do hold recommendations, whose effects are implicit in the intercept. The coefficient on *SELL* is not significant, so sell recommendations do not generate significantly more trading than hold recommendations. However, an F-test of the equality of the *BUY* and *SELL* coefficients is rejected (p-value < 0.01). This result shows that the analyst's recommendation choice significantly affects brokerage market share in the stock in the two weeks after the forecast is released. Thus, both positive stock recommendations and earnings forecasts that deviate from the consensus can significantly increase brokerage-firm market share in the forecast stock. This finding is consistent with Francis and Soffer's (1997) conclusion that analysts' forecasts and recommendations both contain distinct price-relevant information.

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[19] Including *MKT_SHARE* on event day −1 in the dependent variable does not affect the results.

[20] In Hayes' (1998) framework, investors are only affected by short sale constraints when analysts forecast a large negative deviation from consensus. To test the robustness of my result, I examine whether large negative deviations from consensus (rather than all negative deviations from consensus), generate a slope shift in *ABSDEV*. In alternative specifications, I used the 25th, 10th, and 5th percentiles of the forecast deviation from consensus to define *ABSDEV* × *NEG*. In every case, the coefficient on *ABSDEV* × *NEG* is insignificant.

The difference between the effects of *BUY* and *SELL* recommendations on trades flowing through the analyst's broker is large, relative to the effects of deviations from the consensus forecast, *ABSDEV*. Thus, analysts can generate higher trading commissions through positive recommendations than with earnings forecasts that deviate from the consensus.

Analysts' Stock Recommendations and Buy-Side versus Sell-Side Broker Market Share

The TSE data show whether the broker was on the buy-side or the sell-side of a trade. Thus, I calculate each brokerage's daily buy-side *MKT_SHARE* from Equation (7), though using only stock *purchases* in the forecast stock. I calculate each brokerage's daily sell-side *MKT_SHARE* from Equation (7), though using only stock *sales* in the forecast stock. Then I test whether analysts' forecasts, recommendations, and recommendation changes affect the amount of buy and sell trading that flows through the analyst's brokerage firm in the two weeks after publication of the analyst's forecast. The dependent variable in Equation (10a), *SUMBUY*, is the brokerage's buy-side *MKT_SHARE* aggregated over the day 0 through +10 period. In Equation (10b) the dependent variable, *SUMSELL*, is the brokerage's sell-side *MKT_SHARE* aggregated over the day 0 through +10 period:

$$\begin{aligned} SUMBUY = {} & \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \beta_3 BUY + \beta_4 SELL \\ & + \beta_5 UPTOBUY + \beta_6 DOWNTOSELL + \sum_{j=7}^{11} \beta_j INDDUMMY_j \\ & + \varepsilon_i. \end{aligned} \tag{10a}$$

$$\begin{aligned} SUMSELL = {} & \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \beta_3 BUY + \beta_4 SELL \\ & + \beta_5 UPTOBUY + \beta_6 DOWNTOSELL + \sum_{j=7}^{11} \beta_j INDDUMMY_j \\ & + \varepsilon_i. \end{aligned} \tag{10b}$$

Equations (10a) and (10b) include dummy variables for the stock recommendation levels (*BUY* and *SELL*) and dummy variables for recommendation upgrades and downgrades. *UPTOBUY* is a dummy variable that equals 1 if the recommendation is an upgrade to strong buy ($n = 107$). *DOWNTOSELL* is a dummy variable that equals 1 if the recommendation is a downgrade to sell or strong sell (n = 20).[21]

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[21] The results are similar if *UPTOBUY* includes upgrades to buy as well as strong buy (n = 129) or if *DOWNTOSELL* includes downgrades to hold, as well as sell and strong sell (n = 68).

Table 5 presents the results of estimating Equations (10a) and (10b). My key finding is that analyst's earnings forecasts and recommendations have similar effects on both purchases and sales flowing through the analyst's brokerage firm.

First, after including all of the recommendation variables, analysts' forecasts are still important determinants of brokerage market share. In the *SUMBUY* regression, the coefficient on *ABSDEV* is positive (p-value = 0.05), while the coefficient on *UNCERTAINTY* is negative (p-value = 0.07). In the *SUMSELL* regression, the coefficient on *ABSDEV* is positive (p-value = 0.01), while the coefficient on *UNCERTAINTY* is negative (p-value = 0.22). *ABSDEV* and *UNCERTAINTY* are jointly significant in the *SUMBUY* regression (p-value = 0.07) and in the *SUMSELL* regression (p-value = 0.02). Second, the coefficient on *BUY* is positive and significant (p-value < 0.01) in both regressions, and a t-test fails to reject the null hypothesis that the coefficients are equal (p-value = 0.48).[22] Analysts' buy recommendations generate more buy-side market share through the analysts' brokerage firm than do other recommendations, but they also generate more sell-side market share. Conversations with institutional money managers reveal that managers often use the liquidity associated with buy recommendations to sell when they want to rebalance their portfolios. Thus, analysts' buy recommendations generate increased commissions from both client buying and client selling. The dummy variables for upgrades to strong buy and downgrades to sell are jointly insignificant in both the *SUMBUY* regression (p-value = 0.49) and the *SUMSELL* regression (p-value = 0.15). However, the coefficient on *UPTOBUY* is negative and marginally significant (p-value = 0.10) in the *SUMSELL* regression, suggesting that upgrades to strong buy generate less selling activity than do other buy recommendations

Together, the results in Tables 3, 4, and 5 show that both the distance of analysts' forecasts from the consensus and analysts' buy recommendations significantly increase their brokerage-firms' buying and selling activity in the forecast stock.[23]

The Effect of Analysts' Forecast Errors on their Brokerage's Market Share

Finally, I examine whether larger forecast errors (i.e., the difference between an analyst's forecast and I/B/E/S actual earnings) increase the brokerage's share of trading in the forecast stock. Evidence that a larger *ABSDEV* increases the brokerage's share of trading in the forecast stock raises the question whether analysts could increase their commission-related compensation by intentionally adding error to their forecasts to increase the distance between their forecast and the consensus. I cannot directly observe whether analysts add error to their forecasts to generate trading, but I can observe *ex post* forecast errors and examine whether forecast errors generate trading through the analysts' employer.

Table 6 examines whether an analyst's *FORECAST ERROR* is related to her brokerage-firm's market share of trading in the forecast stock. *FORECAST ERROR* is defined as the

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[22] The fact that the *BUY* coefficients are conditioned on the same information (i.e., the independent variables are identical in both regressions) allows me to construct a difference-in-means test using the estimated coefficients and standard errors from Equations (10a) and (10b).

[23] Do buy recommendations increase brokerage-firm trading, or do analysts issue buy recommendations on stocks their brokerage-firms trade heavily? The event-study research design in the paper is limited in its ability to answer this question. In untabulated tests, I examine brokerage-firm trading in the period before the analyst's forecast is released, event days −10 through −2. I find that average daily *MKT_SHARE* for both buy and sell recommendations is significantly lower in the prior period than in the period from event day 0 through +10 (p-value < 0.01), and that *MKT_SHARE* for favorable (buy and strong buy) recommendation is insignificantly different than for unfavorable (hold, sell, or strong sell) recommendations (p-value = 0.16). These results suggest that analysts' recommendations cause brokerage-firm trading to increase. However, a full examination of the direction of causality is left for future work.

TABLE 5
The Effect of Analysts' Stock Recommendation Upgrades and Downgrades on Their Brokerage's Market Share of Buy and Sell Trading

Column 1:

$$\begin{aligned} SUMBUY = {} & \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \beta_2 BUY + \beta_4 SELL \\ & + \beta_5 UPTOBUY + \beta_6 DOWNTOSELL + \sum_{j=7}^{11} \beta_j INDDUMMY_j + \varepsilon_i. \end{aligned}$$

Column 2:

$$\begin{aligned} SUMSELL = {} & \alpha + \beta_1 ABSDEV + \beta_2 UNCERTAINTY + \beta_3 BUY + \beta_4 SELL \\ & + \beta_5 UPTOBUY + \beta_6 DOWNTOSELL + \sum_{j=7}^{11} \beta_j INDDUMMY_j + \varepsilon_i. \end{aligned}$$

	Dependent Variable	Dependent Variable
Independent Variable	***SUMBUY*, Broker Buy-Side Market Share in Event Days 0 through +10**	***SUMSELL*, Broker Sell-Side Market Share in Event Days 0 through +10**
Intercept	0.49 (4.87)	0.49 (5.33)
ABSDEV	5.57 (1.93)	7.40 (2.74)
UNCERTAINTY	−0.13 (−1.87)	−0.09 (−1.23)
BUY	0.33 (4.95)	0.26 (4.26)
SELL	−0.02 (−0.18)	−0.07 (−0.65)
UPTOBUY	−0.02 (−0.20)	−0.14 (−1.71)
DOWNTOSELL	−0.24 (−1.18)	−0.18 (−0.93)
R^2	6.20%	6.35%
Adjusted R^2	4.95%	5.10%
n	835	835

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t-statistics appear in parentheses.

SUMBUY = the sum total of daily brokerage-firm buy-side *MKT_SHARE* traded in event days 0 through +10;

SUMSELL = the sum total of daily brokerage-firm sell-side *MKT_SHARE* traded in event days 0 through +10;

ABSDEV = the absolute value of the difference between the analyst's fiscal year one earnings forecast and the consensus forecast, divided by the stock price on the forecast date;

UNCERTAINTY = the Barron et al. (1998) measure of uncertainty = $\left(1 - \frac{1}{N}\right) D + SE$, in C\$ per share squared. N is the number of forecasts, D is the sample variance of analysts' forecasts, and SE is the sample squared error in the consensus forecast;

BUY = a dummy variable set to 1 if the analyst's recommendation is a buy or a strong buy;

SELL = a dummy variable set to 1 if the analyst's recommendation is a sell or a strong sell;

UPTOBUY = a dummy variable set to 1 if the analyst's recommendation is an upgrade to strong buy;

DOWNTOSELL = a dummy variable set to 1 if the analyst's recommendation is a downgrade to sell or strong sell; and

INDDUMMY = *MINING*, *MANUFACTURE*, *UTILITY*, *TRADE*, and *FINANCIAL* sector dummy variables are included in the regression specification, but for clarity, they are not tabulated.

TABLE 6
The Effects of Analysts' Forecast Errors and Stock Recommendations on Their Brokerage's Market Share of Trading in the Forecast Stock

Regression:

$$SUMSHARE = \alpha + \beta_1 FORECAST\ \ ERROR + \beta_2 BUY + \beta_3 SELL + \sum_{j=4}^{8} \beta_j INDDUMMY_j + \varepsilon_i$$

	Independent Variable
Dependent Variable	Broker Market Share in Event Days 0 through +10
Intercept	0.51 (5.71)
FORECAST ERROR	−2.14 (−1.64)
BUY	0.27 4.91)
SELL	−0.09 (−1.04)
R^2	5.71%
Adjusted R^2	4.80%
n	835

t-statistics appear reported in parentheses.

SUMSHARE = the sum total of daily brokerage-firm *MKT_SHARE* in the forecast stock in event days 0 through +10;

FORECAST ERROR = absolute value of: the analyst's fiscal year one earnings forecast less the I/B/E/S reported actual fiscal year one earnings per share, divided by the price on the forecast date; and

INDDUMMY = the *MINING*, *MANUFACTURE*, *UTILITY*, *TRADE*, and *FINANCIAL* industry dummy variables are included in the regression specification, but for clarity, they are not tabulated.

price-deflated absolute difference between the upcoming year's earnings forecast and the firm's actual earnings as reported on I/B/E/S. Equation (11) regresses *SUMSHARE*, calculated over the day 0 through +10 period, on *FORECAST ERROR*, *BUY*, and *SELL*. I include the recommendation dummy variables because buy recommendations increase brokerage-firm trading, and thus provide an alternative to biasing forecasts for an analyst who wishes to increase her trading-related compensation.

$$SUMSHARE = \alpha + \beta_1 FORECAST\ \ ERROR + \beta_2 BUY + \beta_3 SELL + \sum_{j=4}^{8} \beta_j INDDUMMY_j + \varepsilon_i. \tag{11}$$

The results in Table 6 indicate that *MKT_SHARE* is not positively related to forecast error. Estimating Equation (11) shows that *FORECAST ERROR* is marginally *negatively* related to *SUMSHARE* (p-value = 0.10). This finding indicates that larger absolute forecast errors *reduce* the amount of trading that the analysts' brokerage-firm executes in the forecast stock. The coefficient on *BUY* is positive (p-value < 0.01). Thus, after controlling for forecast error, analysts' buy and strong buy recommendations still significantly increase their brokerage-firms' market share.

The results in Table 6 suggest that biasing forecasts is unlikely to increase analysts' trading-related compensation. Thus, I interpret this result as suggesting that issuing buy and strong buy recommendations is a more effective way for analysts to increase their trading-related compensation than biasing their forecasts.[24,25]

V. CONCLUSION

Using data on brokerage-firm trading from the Toronto Stock Exchange, I examine how analysts' earnings forecasts and their stock recommendations affect their employers' share of trading in the forecast stocks. Analysts' earnings forecasts can significantly increase brokerage-firm market share in the two weeks after the forecast release date. I find that: (1) consistent with Hayes (1998), the further an analyst's earnings forecast diverges from the consensus forecast, the greater the analyst's brokerage-firm's market share of trading, and (2) consistent with the common theoretical prediction that greater uncertainty about the accuracy of a forecast reduces investors' trading, the higher the total uncertainty surrounding the analyst's earnings forecast, the lower the analyst's brokerage-firm's market share of trading.

Contrary to Hayes' (1998) contention, I find that analysts' forecasts below the consensus forecast do not generate less trading commission revenue for the analysts' brokerage firms than do analysts' forecasts above the consensus. I also find no evidence that analysts can generate trade by adding error to their earnings forecasts. Forecast errors produce no significant effect on brokerage-firm market share. However, analysts' buy and strong buy recommendations generate significantly higher trading-commission revenue than other recommendations. I conclude that analysts are more likely to generate trading commissions for their employers by making favorable stock recommendations rather than by upward-biasing their earnings forecasts.

My study generates a set of empirical results that can guide refinements of theory on how trading incentives affect analysts' forecasts and recommendations. To reiterate, I find that analysts' forecasts and stock recommendations exert a significant effect on their brokerage-firms' market share of trading in the forecast stock. This result strengthens the link developed by McNichols and O'Brien (1997) and Hayes (1998) between analysts' actions and their trading-related compensation. Moreover, this result is consistent with Hayes' (1998) contention that the distance of an analyst's forecast from the consensus forecast predicts investors' trading. However, my results are inconsistent with Hayes' (1998) contention that analysts' forecasts below the consensus forecast generate less trading commission revenue than do analysts' forecasts above the consensus. Finally, I report the unpredicted result that analysts' buy recommendations increase both *buying* and *selling* market shares of trading through the analysts' brokerage-firms.

I cannot observe directly whether analysts are biasing their forecasts. As a result, my conclusion that trading incentives are unlikely to affect analysts' forecasts is based partly on the indirect evidence obtained from analyzing *ex post* forecast errors. Neither can I observe trading by investors who receive an analyst's earnings forecast or stock recommendation and choose to trade with a different brokerage-firm. Thus, my results should not be interpreted as representing the total effect of analysts' forecasts and recommendations on their investors' trading. Despite these limitations, my study establishes that investors

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[24] I find similar results using *SUMBUY* or *SUMSELL* as the dependent variable in Equation (11).

[25] Including *ABSVAR* and *UNCERTAINTY* in the regression specification does not affect the inferences. If included in the regression, then the coefficient on *ABSVAR* is always significantly positive, but the coefficient on *UNCERTAINTY* is not significant. The insignificant coefficient on *UNCERTAINTY* is due to the correlation between *UNCERTAINTY* and *FORECAST ERROR*.

trade on the information in analysts' forecasts, and that analysts' favorable stock recommendations increase their employers' market share of trading in the recommended stock. Thus, my results suggest that, as long as the amount of trading done by their employers is a factor in analysts' compensation, then the potential for biased recommendations remains, even if regulators remove analysts' incentives to promote their firms' stock offerings by effectively separating research departments from underwriting departments.

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The Value Relevance and Reliability of Brand Assets Recognized by U.K. Firms

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ABSTRACT: We examine the value relevance and reliability of brand assets recognized by 33 U.K. firms, and the stock price reaction to the announcement of brand capitalization. We find that brand assets are value relevant, i.e., associated with market values. However, the market capitalization rates of brands of firms with low contracting incentives are higher than those of firms with high contracting incentives to capitalize and overstate brand values. Thus, there could be substantial differences in the extent of bias or error in brand valuations of firms with different levels of contracting incentives, i.e., brand asset measures might not be reliable. The stock price reaction during the 21 days surrounding the first announcement of brand recognition is significantly positively associated with the recognized brand amount. However, the brand coefficient is only a small fraction of what would be expected if markets did not impute any value to brands before firms recognized them. Few previous value-relevance studies have examined intangible assets recognized in financial statements, and none have examined the effects of contracting incentives on the reliability of the reported values of intangible assets.

Keywords: *brand assets; intangible assets; contracting incentives; value relevance; reliability.*

Data Availability: *The data are available from the public sources identified in the text.*

JEL Classification Codes: *M4.*

I. INTRODUCTION

Previous studies, including one on brands (Barth, Clement, Foster, and Kasznik 1998), have found that intangible assets are value relevant (see Holthausen and Watts [2001, 21] and their Table 1, which lists eight value-relevance studies on intangible assets). All, except Barth and Clinch (1998), which examines Australian firms' revaluations of both tangible and intangible assets, examine intangible assets that are not recognized in the financial statements. In these studies, the intangible asset measures were constructed by

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researchers or other outside parties who, unlike managers, do not have contracting incentives to bias their valuations.

Our study adds to the literature by examining the relevance and reliability of brands recognized on the balance sheets of 33 U.K. firms beginning in 1985. Brand asset measurement in our sample is subject to managerial discretion—the sample firms acquired brands not in isolation but instead as part of business acquisitions, and valued them separately from goodwill. We examine three research questions: (1) Are recognized brand asset measures value relevant? (2) Do the market capitalization rates of brand assets differ for firms that have high and low incentives to bias the recognized brand amounts? (3) Does the news of brand capitalization convey information to stock markets?

Regressing market values on brand assets, with book values of net non-brand assets, net income, and fixed year- and firm-effects as control variables, we find a positive (+1.24) and significant (at better than the 1 percent level) coefficient on brand assets. Our sample consists of 232 firm-year observations of the 33 brand-capitalizing firms during the 1984–1998 period. As previous studies do (Barth et al. 2001), we interpret a significant coefficient to mean that managers' brand valuations are relevant and, to some degree, reliable.

By showing the value relevance of a recognized intangible asset we address one of Holthausen and Watts's (2001) criticisms of existing value-relevance studies, that results of studies examining outsiders' valuations cannot be generalized to predict the value relevance of managers' valuations of recognized intangible assets. Contracts—such as bonus plans, compensation based on stock price that can be temporarily affected by managerial misstatements, or debt covenants—give managers incentives to bias their valuations of recognized intangible assets. Contracting incentives are likely to be strong in our study because brand assets are a substantial fraction of net assets: the median firm's brand assets are 44.2 percent of book value of equity (including brands). Furthermore, brand accounting was controversial.[1] These factors suggest brand values might not be reliable, yet we find that they are value relevant.

Although value relevance implies that brand asset measures are not totally unreliable, we also test reliability further by investigating differences in brand capitalization rates of firms with strong and weak contracting incentives. If firms with high contracting incentives overvalue brands or introduce greater noise (as Holthausen and Watts [2001, 29] argue), then the stock markets should capitalize their brands at lower rates. Predictable differences in valuation biases or noise would suggest a lack of consensus, violating verifiability, a component of reliability.[2] We base our measures of contracting incentives on Mather and Peasnell's (1991) and Muller's (1999) findings that brand recognition helped firms (1) avoid London Stock Exchange (LSE) rules requiring shareholder approval for large acquisition or disposal transactions and (2) reduce their leverage.[3] We thus partition our sample into subsamples according to (1) whether brand capitalization allowed the firm to avoid the LSE rule for at least one acquisition or disposal transaction, and (2) whether the firm's industry-adjusted debt-to-book-equity ratio was above or below the median.

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[1] Farquhar et al. (1992) quote a statement by the chairman of Britain's Accounting Standards Committee that brand accounting was the major accounting controversy of the last 20 years. Smith (1996) includes it among controversial accounting practices. A London Business School study commissioned by the Institute of Chartered Accountants in England and Wales (Barwise et al. 1989, 78) concludes that the problems facing brand valuation, including subjectivity and lack of precision, are "too intractable to produce useful and meaningful numbers."

[2] Statement of Financial Accounting Concepts (SFAC) No. 2 (FASB 1980) defines verifiability as "the ability through consensus among measurers to ensure that information represents what it purports to represent or that the chosen method of measurement has been used without error or bias." Differences in error and bias among different groups of firms would therefore signify a lack of verifiability. Besides verifiability, SFAC No. 2 mentions representational faithfulness and neutrality as the other components of reliability.

[3] These studies, however, do not provide any evidence regarding value relevance or whether these contracting incentives affect the market capitalization rates of the recognized amounts, the central questions in this study.

Regressing market values on book values, net incomes, brand assets, the variables interacted with a dummy variable based on whether the firm avoided the LSE rule, and firm- and year-effects, we find that brand coefficients for firms with at least one and zero transactions that avoided the LSE rule are 0.35 and 1.94, respectively. Similarly, we find that brand coefficients for high- and low-leverage firms are 0.40 and 1.91, respectively. In both regressions the differences between brand capitalization rates of firms with low and high incentives, 1.59 (= 1.94 – 0.35) and 1.51 (= 1.91 – 0.40), respectively, are statistically significant, while the brand coefficients for firms with high contracting incentives (0.35 and 0.40) are insignificant. When we include dummies for both contracting incentives in the same regression, brand coefficients do not differ significantly between firms that avoided or did not avoid the LSE rule, but the brand coefficient for low-leverage firms remains significantly higher than that for high-leverage firms. These findings suggest that managers' discretionary valuations of intangible assets recognized in financial statements might not be reliable. However, given the differences in market capitalization rates, the markets do seem capable of seeing through the differences in reliability—our findings therefore do not suggest that markets are misled by the lack of reliability.

However, these findings do not address whether markets are misled by nonrecognition of brands, i.e., whether they undervalue brand-intensive firms in the absence of disclosure or recognition of brand asset values. If so, then the news of brand recognition should convey information to stock markets. We find that announcements of brand capitalization do indeed result in positive abnormal returns, on average equaling 12 percent of the value of the brands deflated by market value. Besides undervaluation, the benefits of relaxing contracting constraints can also explain the positive return. Although we attempt to distinguish between the two possible explanations by examining differences in the abnormal returns to firms with high and low contracting incentives, we obtain insignificant coefficient differences, which prevents us from drawing any conclusions. Nevertheless, undervaluation, if any, is small in magnitude—the 0.12 coefficient is much lower than the brand capitalization rate we obtain in our value-relevance regression, 1.24, suggesting that most of the brand values had been capitalized into share prices before firms recognized them.

In related work on the value relevance of discretionary amounts recognized in financial statements, Aboody et al. (1999) examine the value relevance of upward revaluations of *tangible* fixed assets by U.K. firms, and the effect on value relevance of the debt-to-book-equity ratio. The difference in coefficients between high- and low-leverage firms in their study is smaller than in our sample, possibly because we study an intangible asset; such assets are generally considered to be more difficult to value (Barth and Landsman 1995) and hence afford managers more discretion. Also, our sample firms' brand assets are a much higher percentage of book values of net assets, 44.2 percent for the median firm, than are revaluation reserves in Aboody et al.'s (1999) sample (6.5 percent). Our finding therefore indicates that large cross-sectional differences in market capitalization rates could be observed if valuations are subjective and contracting incentives are strong.

Our study contributes not only to the literature on value relevance and reliability of intangible assets, and more generally of recognized discretionary amounts, but also to the policy debate on recognition of intangible assets. An article in *Fortune* (Stewart 2001), for example, opines that the existing accounting system that ignores self-generated intangible assets is like an octogenarian butler: although faithful, he has "lost track of some valuable jewels, paid no attention to the furnace and the water heater, and put the place at risk." *The Economist* (2001), in contrast, argues that intangible assets cannot be reliably valued, and that "soft measures, if required by the authorities, could end up being used to hoodwink investors." In addition, in response to suggestions in "hundreds of recent articles, studies, and consultants' reports" (FASB 2001a, 2), in January 2002 the FASB added to its agenda a project on disclosure of self-generated intangible assets. The proposal (FASB 2001a) states that a secondary goal of the project is to take a step toward recognition of intangible assets. While our results should assist standard setters by providing evidence on value relevance and suggesting problems with reliability, we do not draw explicit policy conclusions because the trade-off between relevance and reliability for standard setting is not precisely quantified (Barth et al. 2001, 81). Moreover, accounting standards are shaped by factors other than value relevance and reliability (Holthausen and Watts 2001; Watts 2002).

We organize the rest of the paper as follows: in the next section we discuss the U.K. accounting practices for brand assets and the resulting contracting incentives. In Section III we describe the sample and provide descriptive statistics. In Section IV we explain our research methods and present our findings. We summarize and conclude the paper in Section V.

II. BRAND ACCOUNTING IN THE U.K. AND CONTRACTING INCENTIVES

U.K. company law and the Statement of Standard Accounting Practice (SSAP) No. 14 (ASB 1978).[4] permit acquired as well as self-generated brands, trademarks, and titles to be recognized on the balance sheet. The Companies Act 1985 allows intangible assets, including self-generated ones other than goodwill, to be recognized at "current cost" (generally taken to be the lower of replacement cost or realizable value). SSAP No. 14 requires that in a business acquisition identifiable intangible assets other than goodwill be recognized at fair value to acquirer. The Companies Act 1985, SSAP No. 14, and SSAP No. 22 (ASB 1984) all cite trademarks and publishing titles as examples of identifiable intangible assets.

Three of our 33 sample firms (Rank Hovis McDougall, The Telegraph, and Goodhead) capitalized self-generated titles and brands (along with other acquired ones), but the other firms capitalized only brands obtained as part of a business acquisition. In contrast to the valuation of brands acquired in isolation, the valuation of self-generated brands and those obtained in business acquisitions is subject to managerial discretion. In the case of self-generated brands, managers have the discretion to determine replacement costs and realizable values, and in the case of brands obtained in business acquisitions, to determine fair value.

Capitalization of self-generated brands and of those obtained in business acquisitions increases owners' equity. Capitalization of self-generated brands increases owners' equity because the amount capitalized would otherwise have been expensed, thereby reducing owners' equity. Capitalization of brands in business acquisitions increases owners' equity because any part of the acquisition price that is not allocated to brands is instead allocated to goodwill (SSAP No. 22), which was immediately written off to owners' equity (the method recommended in SSAP No. 22) by most firms.[5] This difference between the treatment of goodwill and brands[6] enabled U.K. firms to increase owners' equity by recognizing brands separately from goodwill; the higher the brand valuation, the greater the increase in owners' equity.[7]

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[4] SSAP No. 14 was superseded by the Financial Reporting Standard (FRS) No. 2 (ASB 1992a) in 1992. FRS No. 2 does not change the relevant SSAP No. 14 provisions about allocation of the purchase price.

The U.K. Accounting Standards Board issued Financial Reporting Standard (FRS) No. 10 (ASB 1997), effective on December 23, 1998 to replace SSAP No. 22. Like SFAS No. 142 in the USA, FRS No. 10 removes the distinction between goodwill and identifiable intangible assets by not requiring amortization of either kind of intangible asset if it has an indefinite life. From our sample we exclude observations with fiscal years ending on or after December 23, 1998, thus ensuring that brand asset measures in our sample are unaffected by changes in accounting standards.

By increasing owners' equity, brand overvaluation helps U.K. firms avoid LSE rules requiring that companies obtain shareholders' approval for each "material" acquisition or disposal. Although transactions are considered material if any one of five specified ratios[8] exceeds 25 percent, the binding constraint in the majority of cases is the ratio of total acquisition price to the book value of owners' equity of the acquiring firm (Moorhouse 1990). An increase in owners' equity due to brand overvaluation enables firms to undertake larger acquisitions or disposals without seeking shareholder approval[9] and incurring the resulting costs—in terms of both money and time. The monetary costs include professional advisory fees and administrative expenses related to holding an extraordinary general meeting to seek shareholder approval. The delay caused in obtaining shareholder approval is a deal breaker in many potential acquisitions (KATO Communications 1993, 143).

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[5] Although goodwill, too, could be capitalized (SSAP No. 22 ASB [1984, paras. 32–35]), firms preferred to capitalize brands rather than goodwill: brands were not required to be amortized except in the case of value impairment, while goodwill had to be amortized, consequently reducing future earnings. On Compustat Global Vantage we find only 155 firm-year observations with a nonzero goodwill asset, out of over 7,900 firm-year observations of U.K. firms between 1989 and 1997.

[6] In the USA, APB Opinion No. 17 (APB 1970, para. 26), subsequently superseded by SFAS No. 14 (FASB 2001b), required identifiable intangible assets to be recognized separately. SFAS No. 142 (FASB 2001b, paras. B33–B35) requires more categories of intangible assets—those that either arise from contractual or other legal rights, or are separable—to be separately recognized. For example, AOL allocated $10 billion of the $147 billion purchase consideration in its acquisition of Time Warner to brands and trademarks. However, there is no difference in treatment between brands and goodwill—under APB No. 17 both were capitalized and amortized, and under SFAS No. 142 both are capitalized and need not be amortized, but must be tested for impairment.

[7] Maas (1990) opines that capitalization of acquired brands does not affect taxes directly, but could have an adverse indirect effect. Capital gains on sale of some assets can be deferred by purchasing another asset within three years; this rollover benefit is available for goodwill but not for brands. Maas (1990) also opines that capitalized expenditures on self-generated brands may not be tax-deductible.

[8] The five ratios are:

1. The net assets of the acquired/disposed unit (hereafter target) divided by the book value of the owners' equity of the acquiring/divesting firm (hereafter parent).
2. The purchase/sale price divided by the book value of the owners' equity of the parent.
3. Net profits (before taxation and extraordinary items) attributable to the assets of the target divided by those of the parent.
4. The purchase/sale price divided by the market value of all the common shares of the parent.
5. The gross capital of the target divided by the gross capital of the parent. For the parent, gross capital is defined as the aggregate value of common shares (at market value immediately prior to announcement), preferred shares, debt (at market value, if listed), net current liabilities, and all other liabilities such as minority interests and deferred taxes. For the target, a similar aggregation is used except that the amount of the purchase/sale price is used instead of the market value of the common shares being acquired.

[9] A simple numerical example illustrates the benefit. Suppose XYZ company originally has £100 of assets and is all equity financed. It acquires a company for £50 in cash. The tangible assets of the target have a fair value of £20. If the £30 (50 − 20) excess is entirely attributed to goodwill, XYZ's total assets and equity will equal £70 (assets = 100 − cash paid, 50 + tangible assets acquired, 20; owners' equity = 100 − goodwill write-off, 30). On the other hand, if £10 of the purchase price is allocated to brands, then goodwill would equal £20 (50 − tangible assets, 20 − brands, 10). So total assets and equity will equal £80 (assets = 100 − cash paid, 50 + tangible assets acquired, 20 + brands, 10; equity = 100 − goodwill write-off, 20). Subsequent to the acquisition, if XYZ company wants to acquire another company for £18, it will have to seek shareholders' permission in the first case where no brands were recognized (because 18/70 exceeds 25 percent), but not if brands had been recognized (because 18/80 does not exceed 25 percent).

Another contracting incentive for brand overvaluation is that it helps U.K. firms reduce their debt-to-book-equity ratios. This is an important consideration because many U.K. firms have a provision in their charter prohibiting them from borrowing if it increases the debt-to-book-equity ratio beyond a certain limit. In its 1989 annual report, Cadbury Schweppes plc, one of our sample firms, explicitly cited such a provision as a reason for capitalizing brands. U.K. banks also consider the debt-to-book-equity ratio in making their lending decisions (Citron 1992).

Mather and Peasnell (1991) and Muller (1999) report evidence that both contracting considerations motivated firms to capitalize brands. Muller (1999) measures the impact of LSE rules on firms by counting the number of transactions that could avoid the LSE shareholder approval rules in the year subsequent to the first capitalization of brands.

III. SAMPLE AND DESCRIPTIVE STATISTICS

Initially we used *Extel Company Analysis* (one of a family of databases, formerly produced by the *Financial Times*, covering U.K. company financials, share prices, and news) to identify U.K. firms reporting intangible assets in excess of 1 percent of total assets. We determined the nature of the capitalized intangibles from firms' annual reports because brand values are not separately reported in available databases.[10] We include 33 firms that capitalized brands or titles (such as newspaper or magazine titles) in our sample.[11] The earliest instance of capitalization (of titles) was by Reed International in 1985. For the tests that follow, we hand-collected brand values from annual reports and obtained stock prices and accounting data from *Global Vantage*'s 1998 Industrial/Commercial File and Issue File, *Extel Company Analysis*, *Extel Company Research*, *Extel Equity Research*, and *Microview*.

In Table 1, we present a list of the brand-capitalizing firms. The capitalized brands are a substantial percentage of book values of equity for the sample firms—the percentage ranges from a low 1.4 percent to a high 293.2 percent (because of negative owner's equity excluding brands), with a 44.2 percent median. The median sample firm's leverage (debt-to-book-equity ratio) without including brand assets in book value of equity is 1.95. It decreases substantially to 1.16 when brands are included in book values. Market-to-book values of the median sample firms are similarly 5.19 and 2.50 without and with brands. Thus, the brand assets of the sample firms are substantial in comparison to book values of equity and debt; some sample firms are therefore expected to have strong incentives to overvalue brands.

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[10] For example, Compustat's Global Vantage data item 82 records the value of all intangibles; no breakdown is given except for goodwill (item 93).

[11] Some firms give a single figure, without a breakdown, for all intangibles including brands, patents, copyrights, licenses, know-how, etc. We exclude these cases to concentrate on the more controversial assets such as brands and titles. We also exclude firms that capitalized brands or titles acquired individually (not as part of an acquisition) because the valuations are not subject to managers' discretion. We exclude two firms because of special circumstances. One firm, Mirror Group Newspaper plc, was under investigation for serious financial and pension fraud and its shares were suspended from trading over an extended period of time surrounding the date of the first balance sheet capitalizing brands. Another firm, SmithKline Beecham, had a substantial portion of its equity in the form of overseas equity units, and substantial price differences were documented between the two equity types (Frost and Pownall 1996). The relationship between its market value, book value, and brands is thus expected to be unrepresentative.

TABLE 1
Descriptive Statistics for U.K. Firms Capitalizing Brands
Earliest Brand Capitalization and Debt-to-Book Equity and Market-to-Book Ratios

Firm Name	First Balance Sheet Date[a]	Brands/ *BVE*	*D/E* without Brands	*D/E* with Brands	*MVE/BVE* without Brands	*MVE/BVE* with Brands
Reed International	3/31/85	16.4%	1.27	1.06	1.22	1.02
United Newspapers	12/31/85	98.2	130.96	2.42	160.88	2.97
Reckitt and Colman	1/4/86	14.5	1.10	0.94	2.93	2.50
Emap	4/5/86	17.6	1.00	0.82	4.60	3.79
London International	3/31/88	44.2	6.04	3.37	8.38	4.68
Adscene Group	5/28/88	60.1	4.42	1.76	9.02	3.60
Ranks Hovis McDougall	9/3/88	73.0	2.79	0.75	5.19	1.40
Grand Metropolitan	9/30/88	17.3	0.87	0.72	1.51	1.25
Lonrho	9/30/88	10.0	1.73	1.56	1.48	1.33
Guinness	12/31/88	48.2	1.29	0.67	2.01	1.04
Ladbroke	12/31/88	13.9	0.79	0.68	1.07	0.92
United Biscuits	12/31/88	20.7	1.89	1.50	2.96	2.34
WPP	12/31/88	293.2	(5.32)	10.28	(1.92)	3.71
Nu Swift	12/31/88	35.7	10.47	6.73	9.94	6.39
Sterling Publishing	3/31/89	58.5	6.65	2.76	16.74	6.95
Continuous Stationery	3/31/89	82.3	7.93	1.40	16.54	2.93
Southnews	4/1/89	70.6	4.00	1.17	19.99	5.87
Goodhead	5/31/89	37.5	1.95	1.22	2.23	1.39
Daily Mail & General Trust	9/30/89	47.7	3.42	1.79	3.43	1.79
Cadbury Schweppes	12/30/89	51.6	5.33	2.58	8.47	4.09
Jeyes Group	12/30/89	1.4	1.16	1.14	2.77	2.73
Trinity International	12/29/90	18.3	0.98	0.80	2.23	1.82
Blacks Leisure	2/29/92	5.0	1.34	1.27	2.28	2.16
Telegraph	12/31/92	54.8	0.59	0.27	3.19	1.44
Matthew Clark	4/30/93	37.6	0.65	0.40	2.52	1.57
Midland Indep. Newspapers	12/31/94	90.5	5.28	0.50	14.95	1.42
Dalgety	6/30/95	43.0	6.97	3.97	7.36	4.19
Johnston Press	12/31/95	43.7	2.34	1.32	10.05	5.65
Highland Distilleries	8/31/96	31.1	1.50	1.03	3.29	2.26
Scottish Radio Holdings	9/30/96	55.6	1.67	0.74	10.86	4.82
Scottish Media Group	12/31/96	61.4	1.76	0.68	11.61	4.48
Charterhouse Comm.	5/31/97	131.8	(2.44)	0.77	(5.48)	1.74
Newsquest	12/31/97	156.7	(2.04)	1.16	(4.56)	2.59
MEDIAN		44.2	1.95[b]	1.16	5.19[b]	2.50
MEAN		55.8	7.27[c]	1.76	11.66[c]	2.94

[a] For six firms share prices are not available at the date of the first balance sheet containing brand assets, because, e.g., they were not publicly listed at the time. For these firms we report the first balance sheet date with available share price information.

[b] Firms with negative leverage or *MVE/BVE* are treated as having the highest leverage or *MVE/BVE*.

[c] Excluding firms with negative book value of equity.

D/E = debt/equity ratio = total liabilities/book value of shareholders' equity;
MVE = market value of shareholders' equity; and
BVE = book value of shareholders' equity (including brands, except where noted otherwise).

We examined financial statements to determine the sample firms' policies on brand assets (not reported in any table). The basis of valuation is "cost [at acquisition]," "historical cost," or "fair value on acquisition" in most cases. The remaining firms mention "valuation," "directors' valuation," "fair value," or "use value" as the basis. However, very few firms provide details about the methods or assumptions used in the valuation. Only three firms amortized the brand assets. Yet, the carrying values of brand assets do change over time, primarily because of acquisitions and disposals. Descriptive statistics presented in Table 2 show that the coefficient of variation of brands exceeds 10 percent for 20 of the 33 firms. Of the remaining 13, one firm has only a single observation, and six others have no variation in brands. The coefficient of variation of brands is generally increasing with the number of observations; the six firms with no brand variation have only 20 firm-year observations in total.

IV. RESEARCH DESIGN AND FINDINGS

Value Relevance of Brand Assets

Following previous studies (see Barth et al. 2001) we test the value relevance of brand assets by regressing market value of equity on year- and firm-effects, book value of equity (excluding brands), net income, and brand assets using pooled annual time-series and cross-sectional data for all sample firm-years. This regression specification is consistent with the model developed by Ohlson (1995), who shows that it is equivalent to the discounted dividends model under clean surplus accounting,[12] if abnormal earnings follow an autoregressive process. We thus estimate the following regression:

$$\begin{aligned} MVE_{jt} = & \sum_{Y=84}^{98} a_{0Y}YRDUM_Y + \sum_{j=1}^{32} a_{1j}FIRM_j + a_2BVE_{jt} + a_3NI_{jt} \\ & + a_4BRAND_{jt} + e_{jt}, \end{aligned} \tag{1}$$

where:

MVE = market value of equity;
$YRDUM_Y$ = 1 if year = Y, and 0 otherwise;
$FIRM_j$ = 1 for observations pertaining to jth firm, and 0 for others;
BVE = book value of equity (excluding brands);
NI = net income after extraordinary items; and
$BRAND$ = brand assets.

We use net income after extraordinary items because FRS No. 3 (ASB 1992b), effective June 22, 1993, effectively outlawed extraordinary items. Net income before extraordinary items is therefore inconsistent before and after 1993.[13] A significantly positive coefficient a_4 indicates that brand assets are associated with market values after controlling for book

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[12] Although clean surplus does not hold in the U.K. because goodwill is written off directly to owners' equity, Stark (1997) shows analytically that dirty surplus elements can be tolerated in the Ohlson framework if they have no ability to predict future earnings, dividend, or book value. O'Hanlon and Pope (1999) show that dirty surplus elements in the U.K. are not value relevant, thus making it unlikely that they predict future earnings, or dividends, or book value. The absence of clean surplus is therefore unlikely to affect our results. In any case, following previous studies such as Barth, Beaver, and Landsman (1998), we do not rely on the Ohlson framework for our predictions because of its restrictive assumptions such as linear information dynamics.

[13] We also estimated all our regressions using only data for fiscal years ending before June 22, 1993 (119 observations on 25 firms) and our results were qualitatively unchanged.

TABLE 2
Descriptive Statistics
Number of Observations, and Mean, Standard Deviation, and Variation in Brand Values by Firm

Firm Name	n	Brand Value				
		Mean	SD	CV %	Min.	Max.
Emap	13	181.2	172.1	95.0	5.5	479.7
Reckitt and Colman	13	615.0	470.9	76.6	55.8	1295.6
United Newspapers	13	121.8	27.4	22.5	60.0	133.0
Adscene Group	11	16.7	15.3	91.8	2.7	40.0
Southnews	10	14.6	21.5	147.2	2.6	72.5
Sterling Publishing	10	15.4	11.3	73.1	1.8	30.7
United Biscuits	10	174.1	53.4	30.7	106.5	251.0
Daily Mail	10	200.3	35.6	17.8	174.4	260.7
Ladbroke Group	10	326.7	52.7	16.1	276.7	376.7
London International	10	35.6	3.4	9.5	32.0	40.4
Cadbury Schweppes	9	798.1	611.7	76.6	304.0	1689.0
Grand Metropolitan	9	2660.4	965.1	36.3	588.3	3884.0
Jeyes Group	9	0.5	0.2	32.0	0.2	0.7
Goodhead Group	9	11.2	3.0	26.5	6.2	16.0
WPP	9	330.6	58.3	17.6	175.0	350.0
Guinness	9	1388.3	10.0	0.7	1375.0	1395.0
Trinity	8	127.4	141.4	111.0	10.2	354.0
Reed International	8	844.3	648.4	76.8	106.2	1669.7
Matthew Clark	6	9.7	0.0	0.1	9.7	9.7
Ranks Hovis McDougall	5	614.6	105.7	17.2	459.0	740.0
Nu Swift	5	9.5	1.0	10.2	8.5	10.4
Blacks Leisure	4	0.8	0.1	7.2	0.7	0.8
Continuous Stationery	4	4.5	0.0	0.0	4.5	4.5
Lonrho	4	117.0	0.0	0.0	117.0	117.0
Telegraph	4	176.0	0.0	0.0	176.0	176.0
Johnston Press	3	142.5	110.6	77.6	14.9	206.3
Midland Independent	3	112.8	12.7	11.3	99.4	124.7
Scottish Radio Holdings	3	10.9	1.0	9.5	10.2	12.1
Dalgety	3	130.0	0.0	0.0	130.0	130.0
Highland Distillers	3	72.2	0.0	0.0	72.2	72.2
Charterhouse	2	6.4	0.2	2.9	6.2	6.5
Scottish Media Group	2	56.0	0.0	0.0	56.0	56.0
Newsquest	1	320.0			320.0	320.0

n = number of observations;
SD = standard deviation; and
CV = coefficient of variation, i.e., SD/mean.

value and net income. As in other value-relevance studies, we interpret an association as indicating value relevance and at least some degree of reliability (Barth et al. 2001, 81).

We include year dummies to control for fixed year-effects, e.g., effects of macroeconomic factors on stock prices. If correlated with the independent variables, these effects could otherwise bias the regression coefficients. Even if uncorrelated, the effects could still bias the t-statistics by inducing contemporaneous cross-correlation of residuals. Similarly, we include fixed firm-effects to control for any omitted variables that are firm-specific (i.e., constant over time) and for time-series correlation among residuals for the same firm. The use of panel data allows us to include both year- and firm-effects, thereby enabling us to mitigate bias in coefficients as well as standard errors.

Barth and Kallapur (1996) show that scale effects are unlikely to bias the coefficient of interest in cases such as the present one because *BVE* and *NI* are probably more highly correlated with scale than is *BRAND*. Moreover, they show that in some cases deflation worsens bias, so we use undeflated variables in our regression. We use all firm-years beginning with the first year of each firm's brand capitalization, and ending on or before December 22, 1998, the day before the effective date of a new accounting standard on intangible assets, FRS No. 10. This results in a sample of 232 observations. We delete five outliers identified using the *DFBETAS* statistic proposed by Belsley et al. (1980).[14]

Panel A of Table 3 presents correlations among the dependent and independent variables used in the regressions. Although correlations among *BVE*, *NI*, and *BRAND* are high and significant, condition indexes in the regression estimation (not reported in the table) are below 10 and thus multicollinearity does not affect regression coefficients (Belsley et al. 1980). *BRAND* is more highly correlated with *NI* than with *BVE*; this is consistent with some of the brand value being reflected in profitability. Results of estimating Regression Equation (1) presented in Panel B of Table 3 show that the coefficient on *BRAND* is positive (+1.24) and significant at better than the 1 percent level; brands are thus value relevant despite firms' incentives to overvalue them. Our finding that the coefficient value on *BRAND* exceeds 1 is consistent with Jennings et al.'s (1996) findings related to goodwill; our coefficient, however, does not differ significantly from 1 ($t = 0.63$).

In our specification it is difficult to compare the brand coefficient with that on *BVE* because variation in *BVE* at the firm level is likely to be low; the fixed firm-effects might therefore capture much of the effect of *BVE*. Indeed, if we omit firm-effects, then the coefficient on *BVE* increases to 0.83 and is significant. Consequently, we do not draw conclusions from the insignificance of *BVE*. The problem is less likely with *NI*, even though the coefficient on *NI* also increases when we omit fixed firm-effects. The high regression R^2 (0.96) is not attributable to scale effects—the R^2 remains virtually unchanged, and the coefficient on *BRAND* remains significant, when we reestimate the regression after deflating all variables by *BVE*.

We checked the robustness of our results using several alternative specifications described below. First, we perform a randomization test for the purpose of checking whether sample firms' brands and market values are mechanically related because both are large in relation to book values. If the relation is mechanical, then high brand coefficients should be observed even if random brand values are substituted for the true ones (in randomized regressions of market values on the control variables and random brand values), as long as the distribution of random brand values in relation to market and book values is the same as that of true brand values. However, if the observed relation is not mechanical, then the actual brand coefficient should exceed random brand coefficients on average. Thus, in many trials of randomized regressions, the percentage of random brand coefficients exceeding the

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[14] In this and subsequent regressions we eliminate observations that have a *DFBETAS* exceeding 1 in absolute value for any regression coefficient. We also estimate the regressions after deleting observations that have *DFBETAS* exceeding $3/\sqrt{(nobs)}$, as recommended by Belsley et al. (1980). This results in the deletion of 29 to 34 observations in the different regressions, but leaves the results qualitatively unchanged.

TABLE 3
Test of Value Relevance of Brands
Correlations, and Coefficient Values and t-statistics for the Regression

$$MVE_{jt} = \sum_{Y=84}^{98} a_{0Y}YRDUM_Y + \sum_{j=1}^{32} a_{1j}FIRM_j + a_2BVE_{jt} + a_3NI_{jt} + a_4BRAND_{jt} + e_{jt},$$

Panel A: Correlations among Variables Used in Regressions

	BVE	*NI*	*BRAND*
MVE	0.55***	0.81***	0.86***
BVE		0.50***	0.23***
NI			0.74***

Panel B: Regression Coefficient Values and White t-statistics

Variable	Predicted signs	Regression coefficient (White t-stat) n = 227
BVE	+	−0.06
		−0.17
NI	+	2.27
		3.26***
BRAND	+	1.24
		3.27***
Adj R^2		0.96

*, **, *** indicate significance at 10, 5, and 1 percent levels, respectively (one-tailed for coefficients whose signs we predict).

Five outliers, i.e., observations with *DFBETAS* exceeding 1 in absolute value for any coefficient, were deleted for the regression in Panel B.

MVE = market value of equity;
BVE = book value of equity (excluding brands);
NI = net income after extraordinary items;
BRAND = brand assets;
$YRDUM_Y$ = 1 if year = *Y*, and 0 otherwise; and
$FIRM_j$ = 1 for the *j*th firm, and 0 for others.

actual brand coefficient represents a randomization p-value for the null hypothesis that the observed relationship is mechanical.

The relevant property of the distribution of brand values in relation to book and market values that we hold unchanged in randomized trials is the ratio of brand values divided by the excess of market value over book value (call it the brand ratio). To ensure that the distribution of the brand ratios remains unchanged, we randomly shuffle the *brand ratios*, thereby assigning the brand ratio for a given firm-year to any other firm-year observation, and then convert shuffled brand ratios into random brand values. For example, Reed International's brands, book value of non-brand assets, and market value for the year ending 3/31/1985 are 106.2, 542.9, and 664.2, respectively. Its brand ratio therefore equals 106.2/(664.2 − 542.9) = 0.88. Similarly, Grand Metropolitan's brands, book value, and market value for 1988 are 588.3, 2818.7, and 4266.9, respectively. If shuffling results in Reed's 1985 brand ratio being assigned to Grand Metropolitan for 1988 in a particular randomization trial, then Grand Metropolitan's randomized brand value is 0.88*(4266.9 − 2818.7)

= 1274.4. In 100 randomization trials, none of the randomized brand coefficients exceed the actual brand coefficient. The randomization p-value (not reported in any table) is therefore 0.00, and we reject the hypothesis that the significance of brands is mechanical.

Second, previous research finds evidence that the coefficients on *BVE* and *NI* vary as functions of earnings persistence and riskiness. Variables used as proxies for these factors include: persistence of abnormal earnings (estimated in Dechow et al. [1999] using an AR(1) model), return on equity (Burgstahler and Dichev 1997), standard deviation of monthly stock returns, and size, i.e., book value of total assets (Barth, Beaver, and Landsman 1998). We create a dummy variable partitioning the sample firm-years into two groups based on persistence (estimated by an AR(1) model using the time series of each firm's abnormal earnings during the sample period), and reestimate Regression Equation (1) allowing the intercept and coefficients on *BVE* and *NI* to differ for the two groups. We similarly reestimate Regression Equation (1) with dummy variables for each of the other factors interacted with *BVE* and *NI*. We also estimate a model with dummies for all the control variables described above, and their interactions with *BVE* and *NI*, jointly included in the regression equations. The coefficient on *BRAND* is significant in each of these cases at the 1 percent level or better, and does not differ much from the 1.24 value in Table 3—it ranges from 1.12 to 1.36.

Third, we controlled for sales growth rates because brand assets might proxy for them—managers and outside appraisers might be more comfortable justifying high brand values when sales growth is high. The results remain similar.

Fourth, because the sample firms' brand assets are high in relation to book values, one might be concerned about whether our results generalize to other firms that may not have high brand values. We estimated the brand coefficient separately for three groups of firms formed by the ratio of brands-to-book-value of equity. The brand coefficient was significant for each of the groups, which makes it more likely that our results generalize beyond the present sample. Overall, the results of the sensitivity analysis reinforce the conclusion that brand assets are value relevant.

Reliability of Brand Assets

The value relevance finding indicates that any bias or measurement error in brand valuation is not large enough to eliminate its relevance. Nevertheless, the question of whether there are predictable differences in bias and measurement error is likely to be of importance to standard setters because these are features of reliability, a desirable characteristic of accounting numbers (SFAC No. 2). To investigate differences in bias and measurement errors we estimate the market capitalization rates of brands (regression coefficient on *BRAND*) of firms partitioned according to whether their contracting incentives were high or low.

Following our discussion in Section II, our proxies for contracting incentives are (1) whether brand capitalization enabled the firm to avoid shareholder approval for any transaction in the year subsequent to capitalization, and (2) whether the firm's industry-adjusted book-debt-to-equity ratio before brand capitalization is above the sample median. To determine whether firms with low contracting incentives have higher brand capitalization rates, we set dummy variables $D_{LSErule}$ and D_{lolevg} equal to 1 for firms with low contracting incentives, i.e., for firms that did not avoid the LSE rule for any transaction by capitalizing brands, and firms with an industry-adjusted debt-to-book-equity ratio below the sample median. We interact the dummy variables representing contracting incentives with each of the independent variables (book value of equity, net income, and brand assets) to allow the coefficients on the independent variables to differ according to contracting incentives. We thus estimate the following regression, and its restricted versions with only one of the contracting incentives dummies:

$$\begin{aligned} MVE_{jit} = {} & \sum_{Y=84}^{98} b_{0Y}YRDUM_{Y} + \sum_{j=1}^{32} b_{1j}FIRM_{j} + b_{2}BVE_{jt} + b_{3}NI_{jt} + b_{4}BRAND_{jt} \\ & + b_{5}BVE_{jt}{*}D_{LSErule} + b_{6}BVE_{jt}{*}D_{lolevg} + b_{7}NI_{jt}{*}D_{LSErule} + b_{8}NI_{jt}{*}D_{lolevg} \\ & + b_{9}BRAND_{jt}{*}D_{LSErule} + b_{10}BRAND_{jt}{*}D_{lolevg} + e_{jt}, \end{aligned} \qquad (2)$$

where:

$D_{LSErule}$ = a dummy variable equaling 0 if brand capitalization enabled a firm to avoid the LSE rule regarding shareholder approval for one or more transactions during the year subsequent to capitalization. Because brands increase owners' equity, the denominator in the first two of the five ratios enumerated in footnote 8 (net assets of the acquired/disposed entity divided by the owners' equity of the parent firm or the net purchase consideration divided by the owners' equity of the parent firm), we consider a transaction to have avoided the LSE rules if either of those ratios exceeds 25 percent without including brand assets, and both ratios are lower than 25 percent after including brands. Further, we ignore any transaction with a consideration-to-market-capitalization ratio exceeding 25 percent because those transactions would need shareholder approval regardless of brand capitalization. Twelve of the 33 sample firms had transactions that avoided the LSE rules as a result of brand capitalization; and

D_{lolevg} = a dummy variable equaling 1 if the sample firm's industry-adjusted debt-to-book-equity (excluding brands) ratio in the first year of brand capitalization is below the sample median. We define an industry as firms with the same three- or four-digit U.K. SIC code. For consistency, we exclude intangible assets from book value of equity in calculating debt-to-book-equity ratio for firms in the industry group.

Once firms have decided to capitalize brands they apply their chosen accounting method consistently in future periods, instead of freshly deciding each period whether to capitalize brands. We therefore calculate each contracting-incentives dummy ($D_{LSErule}$ and D_{lolevg}) at the firm level, using information at the time of the first brand capitalization. Therefore, all firm-year observations for a given firm *j* have the same value of the contracting-incentives dummy.

Coefficient b_4 measures the brand assets' market capitalization rate for firms with at least one acquisition/disposal transaction that avoided the LSE rules and had high leverage; and b_9 and b_{10} measure how this rate differs for firms without transactions that avoided the LSE rules or had low-leverage, respectively. If firms with greater contracting incentives overstate brands, then the market capitalization rate for their brands will be lower, and those for the brands of firms with low contracting incentives will be higher. Accordingly, b_9 and b_{10} will exceed zero.

Holthausen and Watts (2001, 29) suggest that managers might introduce noise as well as bias to mislead auditors and stock markets. Such noise would lead to errors in brand measures, which would bias the estimated regression coefficient toward zero in a multiple regression if other regressors are measured without error (Garber and Klepper 1980). Because the brand coefficient is expected to be positive, bias toward zero implies a smaller coefficient for firms with greater contracting incentives. Thus, the effect of a larger error in brand measures of firms with high contracting incentives is likely to be the same as that of overstating brands.[15]

We include $BVE*D_{LSErule}$, $BVE*D_{lolevg}$, $NI*D_{LSErule}$, and $NI*D_{lolevg}$ to ensure that the estimated coefficients on $BRAND*D_{LSErule}$ and $BRAND*D_{lolevg}$ are unaffected by any association between stock prices and interactions of the contracting dummy with *BVE* and *NI*. The firm dummies included in the regression are finer than the contracting dummy (whose value depends on whether an observation pertains to a firm that is classified into the high- or low-contracting-incentives group). Therefore, we do not include contracting dummies in the regression—they would be collinear with some of the firm dummies if we did. In addition to the firm dummies, as in estimating Regression Equation (1), we include year dummies to capture fixed-year effects. The sample, as before, consists of 232 firm-year observations from which we delete two outliers identified using the *DFBETAS* statistic proposed by Belsley et al. (1980).

Panel A of Table 4 reports the correlations between the contracting dummies and the other regression variables. Although $D_{LSErule}$ and D_{lolevg} are significantly correlated (0.36), the correlation is far from perfect. We report results of estimating Regression Equation (2) and its restricted versions deleting terms involving D_{lolevg} and $D_{LSErule}$ in Panel B of Table 4. When estimated separately in the restricted regressions, the coefficients on $BRAND*D_{LSErule}$ and $BRAND*D_{lolevg}$ are positive, 1.59 and 1.51, respectively, and significantly different from zero at $p < 0.01$. The effective number of observations in each case—the number of observations with contracting-incentives dummy equal to 1—is 153 and 117 respectively for $D_{LSErule}$ and D_{lolevg}.

The significant coefficients indicate that the market's brand capitalization rate for firms with lower contracting incentives to overstate brands (firms with no transactions that escaped the LSE requirements, or with low leverage) is higher than that for firms with higher contracting incentives. The coefficient on *BRAND* for firms with transactions that avoided the LSE rule and had high leverage are 0.35 and 0.40, respectively, and these values are not statistically significant. The market's capitalization rates for brand assets of firms with no transactions that avoided the LSE rule is 1.94 (0.35 + 1.59); and that for low-leverage firms is 1.91 (0.40 + 1.51). In the unrestricted model, when we include terms involving both $D_{LSErule}$ and D_{lolevg}, the coefficient on $BRAND*D_{LSErule}$ is insignificant, but the coefficient on $BRAND*D_{lolevg}$ remains significant. However, these results could be affected by multicollinearity: the coefficient of correlation between $BRAND*D_{LSErule}$ and $BRAND*D_{lolevg}$ is 0.94 (not reported in any table).

The point estimates of the coefficients in the restricted regressions therefore indicate that the brand capitalization rates for firms with lower contracting incentives are five to six times ($1.94/0.35 = 5.5$ and $1.91/0.40 = 4.8$) the capitalization rates of firms with high contracting incentives. Thus, the differences in brand capitalization rates of firms with high and low contracting incentives are statistically as well as economically significant, suggesting that brand asset measures might lack reliability for firms with high contracting incentives. This conclusion is contrary to that of Barth and Clinch (1998) who find that discretionary revaluations of intangible assets in Australia are as value relevant as

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[15] The regression coefficient remains biased toward zero even if other variables are measured with error, as long as those errors are small (i.e., have low variance). However, if the error in measuring other variables is large, then the effect of measurement error on the coefficient could be opposite to that of the bias, weakening our ability to find the hypothesized results.

TABLE 4
Test of Reliability—Value Relevance of Brands in Subsamples Partitioned by Dummies for Contracting Incentives
Correlations, and Coefficient Values and White t-statistics for the Regression

$$MVE_{jt} = \sum_{Y=84}^{98} b_{0Y}YRDUM_Y + \sum_{j=1}^{32} b_{1j}FIRM_j + b_2BVE_{jt} + b_3NI_{jt} + b_4BRAND_{jt} + b_5BVE_{jt}{*}D_{LSErule} + b_6BVE_{jt}{*}D_{lolevg} + b_7NI_{jt}{*}D_{LSErule} + b_8NI_{jt}{*}D_{lolevg} + b_9BRAND_{jt}{*}D_{LSErule} + b_{10}BRAND_{jt}{*}D_{lolevg} + e_{jr}$$

Panel A: Correlations among Variables Used in Regressions[a]

	$D_{LSErule}$	D_{lolevg}
MVE	0.05	0.31***
BVE	0.27***	0.35***
NI	0.03	0.26***
BRAND	0.03	0.28***
$D_{LSErule}$		0.36***

Panel B: Regression Coefficients and White t-statistics

Variable	Predicted Signs	Model 1 n = 230	Model 2 n = 230	Model 3 n = 230
BVE	+	−0.42 (−1.10)	−0.60 (−1.52)*	−0.41 (−1.13)
NI	+	7.19 (5.16)***	6.72 (5.25)***	8.55 (7.24)***
BRAND	+	0.35 (0.94)	0.40 (1.05)	0.39 (1.03)
$BVE{*}D_{LSErule}$	?	1.03 (1.89)**		−1.07 (−1.49)*
$BVE{*}D_{lolevg}$	?		1.27 (2.29)**	2.13 (2.73)***
$NI{*}D_{LSErule}$	?	−7.96 (−5.19)***		−3.68 (−2.79)***
$NI{*}D_{lolevg}$	?		−7.65 (−5.45)***	−5.87 (−5.30)***
$BRAND{*}D_{LSErule}$	+	1.59 (2.98)***		(−0.20) (−0.31)
$BRAND{*}D_{lolevg}$	+		1.51 (2.80)***	1.76 (2.49)***
Adj R^2		0.96	0.96	0.96

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*, **, *** indicate significance at 10, 5, and 1 percent levels, respectively (one-tailed for coefficients whose signs we predict).

[a] See Panel A of Table 3 for correlations among the other regression variables.

In each model two outliers, i.e., observations with *DFBETAS* exceeding 1 in absolute value for any coefficient, were deleted.

White heteroscedasticity-adjusted t-statistics in parentheses.

MVE = market value of equity;
BVE = book value of equity (excluding brands);
NI = net income after extraordinary items;
BRAND = brand assets;
$YRDUM_Y$ = 1 if year = *Y*, and 0 otherwise;
$FIRM_j$ = 1 for *j*th firm, and 0 for others;

(*continued on next page*)

TABLE 4 (continued)

$D_{LSErule}$ = a dummy variable equaling 0 if brand capitalization enabled a firm to avoid the LSE rule regarding shareholder approval for one or more transactions during the year subsequent to capitalization. A transaction is considered to have avoided the LSE rules if either the assets ratio (ratio of net assets of the acquired/disposed entity divided by the owners' equity of the parent firm) or the consideration ratio (net purchase consideration paid or received divided by the owners' equity of the parent firm) exceeds 25 percent without including brand assets, but both ratios are lower than 25 percent after including brands. We ignore any transactions with a consideration-to-market-capitalization ratio exceeding 25 percent because those transactions would need shareholder approval regardless of brand capitalization. Twelve of the 33 sample firms had transactions that avoided the LSE rules as a result of brand capitalization; and

D_{lolevg} = a dummy variable equaling 1 if the sample firm's industry-adjusted debt-to-book-equity (excluding brands) ratio in the first year of brand capitalization is below the sample median. We define an industry as the same three- or four-digit U.K. SIC code. For the sake of consistency, we exclude intangible assets from book value of equity in calculating debt-to-book-equity ratio for firms in the industry group.

are revaluations of tangible assets. However, they do not condition their analysis on contracting incentives—ours is therefore a stronger test of reliability.

Another study, Aboody et al. (1999), finds modest differences in value relevance of discretionary revaluations of tangible fixed assets in the U.K. depending on firms' debt to equity ratio—in regressions with stock price as the dependent variable, they find that the coefficient on the revaluation balance is 0.44, and that on the revaluation balance interacted with the debt-to-book-equity ratio is −0.02. Aboody et al. (1999) use a continuous debt-to-equity-ratio measure unlike the dummy we use. Nevertheless, we can estimate that the difference between the coefficients of high- and low-leverage firms in their sample is definitely below 0.10 (0.02*5); they report that the debt-to-book-equity ratio does not exceed 5 for any firm. The 0.10 coefficient is smaller than in our study (1.51) in absolute as well as in relative terms (0.10/0.44 versus 1.51/1.91). One possible reason is that we examine the value relevance of an intangible asset; these assets are generally more difficult to value (Barth and Landsman 1995) and, hence, afford managers more discretion. Second, the median firm's revaluation reserves are only 6.5 percent of book values in their sample, while the median firm's brand assets are 44.2 percent of book values in ours. Our finding, therefore, adds to the literature by showing that large cross-sectional differences in market capitalization rates could be observed if valuations are subjective and contracting incentives are strong.

The coefficients 0.35, 0.40, and 0.39 on *BRAND* in different versions of Regression Equation (2) are statistically insignificant at conventional levels. This is consistent with lack of reliability overwhelming their value relevance. However, a conclusion about lack of value relevance based on insignificant coefficients would be valid only if the test has sufficient power. Because the sample sizes are relatively small, we refrain from drawing conclusions based on statistically insignificant coefficients.

One potential problem with the use of the $D_{LSErule}$ variable is that errors in brand valuation could affect whether the firm subsequently avoids the LSE requirements. That is, if firms' brand valuations are unbiased but have some error, then firms with positive valuation error (overvaluers) will have lower brand capitalization rates (if markets undo the effects of the error) and will be more likely to avoid the LSE requirements in the future. Thus, the finding of lower brand capitalization rates for firms with $D_{LSErule} = 0$ could be attributable to valuation errors rather than contracting incentives. The small and insignificant correlation between $D_{LSErule}$ and *BRAND* (0.03, reported in Panel A of Table 4) casts doubt on the validity of this explanation. Nevertheless, in another attempt to rule out this explanation[16] we use an alternative measure instead of $D_{LSErule}$, namely whether the firm needed shareholder approval for any acquisition during the year *prior to* the date of the balance sheet in which brands were first capitalized. This measure proxies for the probability of a firm being subject to the LSE requirements in future, assuming that past acquisitions are good indicators of future acquisitions. Specifically we define $D'_{LSErule} = 1$ if the firm had no transactions that required shareholder approval under the LSE rules during the year ending on the date of the first balance sheet containing capitalized brands. The coefficient $BRAND{*}D'_{LSErule}$ remains significant at better than the 1 percent level using this alternative specification.

Our findings regarding the significance of coefficients on $BRAND{*}D_{LSErule}$ and $BRAND{*}D_{lolevg}$ are largely unaffected when we perform the robustness tests described for the unpartitioned sample. There is, however, one exception: the randomization p-values are 0.15 and 0.14.[17] Our finding that brand capitalization rates of firms with high and low contracting incentives differ must therefore be interpreted with caution. Also, if brand valuations indeed lack reliability, then our evidence does not suggest that markets are thereby misled—on the contrary, our findings are based on differences in market capitalization. Investigation of market mispricing would require an examination of future returns, which is beyond the scope of this study.

Stock Price Effects of Brand Recognition

Previous evidence suggests that markets undervalue intangible-asset-intensive firms in the absence of recognition (Aboody and Lev 2000; Lev et al. 2000; Chan et al. 2001; Lev 2001). Lev et al. (2000) and Chan et al. (2001) report positive risk-adjusted returns to portfolios of stocks of firms with high R&D growth rates and high R&D-to-market-value ratios, respectively. Aboody and Lev (2000) find that trades by insiders in R&D-intensive firms are three to four times as profitable as are trades by insiders in non-R&D-intensive firms, thus suggesting greater information asymmetries in R&D-intensive firms. We examine the stock price reaction to brand capitalization announcements to test whether brand-intensive firms were undervalued before they capitalized brands.

With regard to brands, undervaluation could result because markets impute conservative brand valuations to all firms in the absence of disclosure. Alternatively, markets could be unbiased, on average, and firms whose brands are undervalued could selectively resort to brand recognition to signal their true values. In either case, if markets undervalue brands before firms recognize them, then brand recognition enables firms to overcome information asymmetries.

If brands were undervalued before firms recognized them, then news of brand recognition must convey positive information to markets. Besides undervaluation, positive information content could also be explained by the benefit to firms of having relaxed contracting constraints. In contrast, Smith (1996) considers brand capitalization a controversial accounting practice and its use a signal of an underlying weakness in a firm's financial position. If markets interpret brand capitalization in this way, then we should observe a negative stock price reaction to the news of brand capitalization.

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[16] We measure leverage using the book value of equity excluding brands; the leverage-based contracting incentive measure is therefore unaffected by this problem.

[17] The purpose of the regression equations is to assess whether the brand coefficient differs according to contracting incentives. Accordingly for the randomization test we randomly assign firms to $D_{LSErule} = 0$ and $D_{LSErule} = 1$ categories, and leave other variables unchanged (similarly for D_{lolevg}).

We are able to estimate the market reaction for only 24 of the 33 sample firms. For six firms the earliest announcement was before its stock was publicly traded, for two firms the stocks were newly listed and previous returns for estimating the market model were not available, and in one other case we cannot determine the date of earliest announcement of brand values. To determine the market reaction to brand announcements, we regress market-and-risk-adjusted abnormal returns (estimated using market model betas calculated over the 60 days ending 30 days before the event date) on *BRAND/MVE*. Our specification is as follows:

$$AR_{jt} = d_0 + d_1(BRAND_{jt}/MVE_{j,t-1}) + e_{jt}, \qquad (3)$$

where AR_{jt} is the announcement period (days -10 to $+10$)[18] abnormal returns for the *j*th firm in year *t* (estimated as the difference between actual returns and expected returns based on market model parameters estimated over the 60 trading days ending 30 days before the announcement).

Results presented in Table 5 show that the coefficient on *BRAND/MVE* is +0.12 and significant at better than the 1 percent level.[19] The coefficient should equal that on *BRAND* in Regression Equation (1) if ten days before news of brand capitalization the markets expected *zero* brand values, and did not receive news about any other variables during the 21-day returns accumulation period;[20] under these assumptions, Regression Equation (3) can be derived from Regression Equation (1) by first-differencing and deflating by market value. The comparison of coefficients, however, indicates that the 0.12 coefficient on *BRAND/MVE* in Regression Equation (3) is much lower than the 1.24 coefficient on *BRAND* in Regression Equation (1). Thus, the market had already capitalized all but 10 percent (= 0.12/1.24) of the brand values into prices before brands were actually capitalized by firms. Thus, undervaluation of brands, if any, is small in economic magnitude.

The significantly positive return to news of brand capitalization is consistent with either undervaluation or the benefit to firms from relaxing contracting constraints through brand capitalization;[21] it is inconsistent with brand recognition being interpreted as a sign of weakness. To evaluate the constraint relaxation explanation, we interact *BRAND/MVE* with contracting dummies ($D_{LSErule}$ and D_{lolevg}) and include the interaction terms in the regression, as follows:

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[18] Results for other announcement periods such as days -2 to $+1$ and -5 to $+5$ were slightly weaker.

[19] A randomization test similar to the one for the value-relevance regression indicates that the coefficient on *BRAND/MVE* is significant at the 4 percent level. We randomly shuffled *BRAND/MVE* values.

[20] In fact, 12 of the 24 announcements were concurrent with earnings announcements. We conduct additional sensitivity tests by adjusting the abnormal returns for earnings levels and changes (following Easton and Harris 1991) in these cases. We determine response coefficients on earnings levels and changes, required for the adjustment, by regressing abnormal returns on earnings levels and changes using pooled cross-section and time-series data for all available years for the 12 firms. Our results are similar to those reported.

[21] Actually, the requirement for shareholder approval was meant to protect shareholders—it is therefore not clear whether shareholders benefit when firms are able to relax this constraint. However, if the constraint had resulted in approval being required even for very small transactions due to equity depletion resulting from repeated goodwill write-offs, shareholders might benefit. In any case, a positive brand coefficient suggests that any unfavorable effect on shareholders because managers were able to relax the constraint is not the dominant explanation for the market returns.

TABLE 5
Share Price Reactions to Announcements of Brand Capitalization: All Firms, and Subsamples Partitioned by Contracting Incentives Coefficient values and White t-statistics for the Regressions

$$AR_{jt} = d_0 + d_1(BRAND_{jt}/MVE_{j,t-1}) + e_{jt}$$

and

$$AR_{jt} = f_0 + f_1(BRAND_{jt}/MVE_{j,t-1}) + f_2D_{LSErule} + f_3D_{lolevg} + f_4\,(\mathrm{BRAND}_{jt}/MVE_{j,t-1}{*}D_{LSErule}) + f_5(BRAND_{jt}/MVE_{j,t-1}{*}D_{lolevg}) + e_{jt}.$$

Variable	Predicted Signs	Model 1 (n = 24)	Model 2 (n = 24)	Model 3 (n = 24)	Model 4 (n = 24)
Intercept	?	−0.01 (−0.20)	0.02 (0.62)	−0.06 (−1.36)	−0.05 (−1.00)
BRAND/MVE	+/−	0.12 (3.21)***	0.08 (2.76)***	0.16 (3.35)***	0.15 (3.16)***
$D_{LSErule}$	?		−0.05 (−0.97)		−0.22 (−1.89)*
D_{lolevg}	?			0.07 (1.13)	0.25 (2.13)**
BRAND/MVE * $D_{LSErule}$	+		0.21 (1.07)		1.85 (1.88)**
BRAND/MVE * D_{lolevg}	+			−0.00 (−0.01)	−1.77 (−1.82)**
Adj R^2		0.02	0.03	0.08	0.06

*, **, *** indicate significance at 10, 5, and 1 percent levels, respectively (one-tailed for coefficients whose signs we predict).

AR_{jt} = brand capitalization announcement period (days −10 to +10) abnormal returns for the *j*th firm in year *t* (estimated as the difference between actual returns and expected returns based on market model parameters estimated over the 60 trading days ending 30 days before the announcement);

$BRAND_{jt}$ = brand assets at the end of year *t*; and

$MVE_{j,t-1}$ = market value of equity at the end of year $t-1$.

$D_{LSErule}$ and D_{lolevg} are as defined in Table 4.

$$AR_{jt} = f_0 + f_1(BRAND_{jt}/MVE_{j,t-1}) + f_2D_{LSErule} + f_3D_{lolevg} + f_4(BRAND_{jt}/MVE_{j,t-1}{*}D_{LSErule}) + f_5(BRAND_{jt}/MVE_{j,t-1}{*}D_{lolevg}) + e_{jt}, \tag{4}$$

where all variables are as defined before.

Results presented in Table 5 show that in the restricted versions of Equation (4) with only one or the other contracting dummy, the coefficients on *BRAND/MVE* * $D_{LSErule}$ and *BRAND/MVE* * D_{lolevg}, 0.21 and −0.00, respectively, do not differ significantly from zero; the contracting constraint relaxation explanation predicts positive coefficients. Because of their insignificance, we are unable to distinguish between the two possible explanations for positive returns at the announcement of brand capitalization. Thus, we cannot rule out undervaluation of brands but, as argued above, the magnitude of undervaluation is small. In the unrestricted version, the coefficients on *BRAND/MVE* * $D_{LSErule}$ and *BRAND/MVE* * D_{lolevg}, 1.85 and −1.77, respectively, are both significant at the 5 percent level but in opposite directions. Because the coefficient of correlation between $BRAND/MVE * D_{LSErule}$ and $BRAND/MVE * D_{lolevg}$ is very high (0.94), we do not draw any conclusions from this result.

V. CONCLUSIONS

This study examines the value relevance and reliability of brand assets recognized by 33 U.K. firms beginning in 1985. Most previous studies on the value relevance of intangible assets have examined valuations of outside parties rather than managers who could be subject to contracting incentives to bias recognized amounts. The only other study that examines recognized intangible assets, Barth and Clinch (1998), does not examine the effects of contracting incentives on value relevance. In addition, previous studies on brand recognition in the U.K. (e.g., Muller 1999) have examined the incentives for brand recognition, but not the effects of these incentives on the value relevance and reliability of the recognized brand values.

Our results suggest that recognized brand values are value relevant despite managers' incentives to overvalue them. These findings are robust in several sensitivity tests. However, market capitalization rates of brands of firms with low contracting incentives—firms with no transactions that avoided the LSE rules requiring shareholder approval for acquisitions /disposals, and firms with industry-adjusted leverage below the sample median—are substantially higher than those of firms with high contracting incentives. The difference in market capitalization rates indicates differences in the amount of bias or error in brand valuations of different groups of firms, suggesting that brand asset measures lack reliability for firms with high contracting incentives. These results must, however, be interpreted with caution because in a randomization test we perform for sensitivity analysis, the differences are significant at only the 15 and 14 percent levels. Another caveat is that our findings are based on market capitalization rates; they therefore do not indicate that markets are misled by the lack of reliability.

Previous studies that investigate the effects of contracting incentives on the value relevance of managers' discretionary valuations (e.g., Aboody et al. [1999], which examines managers' revaluations of tangible fixed assets) find much smaller differences in capitalization rates. Brand assets in our sample are higher as a proportion of total assets than is the revaluation balance in Aboody et al.'s (1999) sample. Furthermore, the valuation of intangible assets such as brands is likely to be more discretionary. Subject to caveats mentioned before, our study therefore contributes to the literature by showing that substantial differences in market capitalization rates of recognized assets could be observed for different groups of firms when contracting incentives are strong and the valuations are subjective.

We also find a significantly positive association between market returns during the 21-day period surrounding the first announcement of brand asset values. The positive association could result either from undervaluation of brands by markets prior to recognition, as suggested by Lev (2001), or from the relaxation of contracting constraints. Although we attempt to distinguish between the two explanations for a positive association, insignificant coefficients prevent us from doing so. We are, thus, unable to rule out the undervaluation explanation, but the magnitude of undervaluation is small.

By examining the value relevance and reliability of intangible assets, we contribute to the policy debate on recognition of intangible assets. However, we do not draw any policy conclusions because our evidence suggests relevance as well as a possible lack of reliability, and the trade-off between these considerations, is not precisely quantified (Barth et al.

2001). Besides, accounting standards are shaped by factors other than value relevance and reliability (Holthausen and Watts 2001; Watts 2002).

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Discipline with Common Agency: The Case of Audit and Nonaudit Services

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ABSTRACT: Using a common agency model, we investigate the interactions of a utility-maximizing auditor (the agent) with managers (who hire the agent for nonaudit services) and shareholders (who hire the agent for conducting an audit) of the same firm. In a single-period model, managerial discretion over consulting and other nonaudit service fees can influence auditors to issue unqualified opinions on reports that are more favorable than warranted. Shareholders, represented by an audit committee, cannot recover truth-telling. Removing the current restriction on *contingent* audit fees allows audit committees to offset the incentives provided by management and instead provide the auditor incentives to accept only truthful reports. Extending the model to a multi-period framework, the audit committee can motivate truth-telling by making retention decisions that are contingent on outcome. Auditors will consider the impact of over-reporting on their ability to generate future audit fees from the same client.

I. INTRODUCTION

This paper addresses the independence problems that can arise when firms hire the same agent to provide audit and nonaudit services. We model the relationship between management, the audit committee, and auditors as a common agency, with managers and the audit committee as principals and the auditor as the agent. Managers may influence the outcome of audit reports by awarding nonaudit (e.g., consulting) service contracts in exchange for favorable audit reports. When audit fees are restricted to be noncontingent (i.e., unrelated to outcomes), the audit committee may not be able to recover truthful audit reports. Prohibiting the joint provision of audit and nonaudit services might solve the problem, but at what cost? Restricting audit firms from providing consulting services to audit clients destroys the audit firms' abilities to achieve potentially valuable economies of scope (Antle and Demski 1991). In addition, audit firms argue that a smaller market for the services of the audit firms will make it harder for them to attract top talent and the quality of audits will suffer (McNamee et al. 2000). Instead of an exclusionary ban, we

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suggest eliminating the restriction on noncontingent audit fees, allowing the audit committee to write contracts with their auditor that depend on the outcome of the audit report. We also analyze how auditor-retention strategies in an ongoing interaction can mimic explicitly contingent fees. Then, without eradicating the synergies gained from acquiring audit and nonaudit services from the same provider, the audit committee can recover truthful reporting.

For more than 60 years, auditors have played a large role in maintaining the integrity of financial reports. "Without accountants to ensure the quality and integrity of financial information, the markets for capital would be far less efficient, the cost of capital would be far higher, and our standard of living would be lower" (Wallman 1995). Rules defining and regulating auditor independence have been an integral part of the securities laws since the 1930s. While a precise definition of independence is the source of academic and professional debate, independence in auditing is generally understood to mean an unbiased viewpoint when performing audit tests, evaluating the results, and issuing the audit report.

In the 1980s, a succession of business failures that were viewed as audit failures prompted researchers and practitioners to investigate the accounting profession and public confidence. In surveys of public confidence, there was no noticeable difference in the perceived audit quality of firms whose auditors did and did not also provide nonaudit services.[1]

In the past decade, large accounting firms have, more than ever, consolidated and transformed themselves into multidisciplinary professional service organizations and the issue of auditor independence has re-emerged. Arthur Levitt, former chairman of the Securities and Exchange Commission, asks "How can auditors remain independent when their [audit] fee income is just 30 percent of their firms' total revenues?"[2] Sutton (1997) argues that despite the many studies by the public accounting profession that find no conclusive evidence that nonaudit services compromise auditor independence, doubt about the objectivity and independence of auditors persists. Fundamentally, "[there] is an inherent skepticism about how close the relationship between the auditor and the management of the audit client can be without creating, in fact or in perception, a mutuality of interest that could impair the auditor's independence" (Sutton 1997). These concerns rest on the notion that audit and nonaudit services are provided to a different (and potentially misaligned) set of constituents, namely the audit committee and management.

In June 2000, the SEC drafted a proposal to modernize the rules for auditor independence. The proposal included limits on the scope of services provided by audit firms to their audit clients.[3] This portion of the proposal was the most contentious, and elicited many responses from auditing professionals and their clients. Following several highly publicized corporate failures, Congress passed the Sarbanes-Oxley Act of 2002, which formalized the requirements for communication between auditors and audit committees, restricted the types of nonaudit services that auditors may provide and required CEOs and CFOs to certify the financial statements of their firms. While Sarbanes-Oxley may increase the costs of misreporting, it does not eliminate all independence problems. In this paper, we demonstrate the ability of shareholders to discipline management, without regulatory intervention. Rather than being compensated more for favorable reports, audit committees will align auditors' incentives with those of the shareholders they represent by rewarding audited reports that show relatively low levels of earnings. The incentives provided by the audit committee are balanced against auditor's desire to curry favor with management to gain nonaudit service revenues.

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[1] For example, a recent survey commissioned by the Independence Standards Board indicated that respondents believed that the evolution of audit firms into consulting fields was logical and the provision of most consulting services was not likely to create a real problem of independence (Independence Standards Board 1999).

[2] Audit firms' fees from consulting services for their SEC clients increased from 17 percent of audit fees in 1990 to 67 percent in 1999 (Panel on Audit Effectiveness 2000).

[3] A subset of the members of the Panel on Audit Effectiveness suggested an exclusionary ban on nonaudit services in their report.

In addition to the controversial debate over barring auditors from providing nonaudit services to audit clients, others have argued strenuously for mandatory auditor rotation. This suggestion rests on the premise that long-term auditor-client relationships lead to an impairment of objectivity. New auditors would provide a fresh (and more skeptical) look at the client. Opponents of auditor rotation argue that continuity makes for better and less costly audits, and that there is rotation of staffers in the normal course of business. In this paper, we discuss how the repeated nature of auditing encourages truthful reporting. With an ongoing interaction, contingent auditor-retention schemes can mimic the effects of contingent fees that we analyze in a single-period setting.

This paper contributes to the theory related to incentives for auditors and proposes novel remedies for a hotly debated policy topic. We model and discuss a less regulatory approach (eliminating restrictions on forms of contracts) rather than a more regulatory approach (bans on activities) to restoring the balance between incentives for exaggeration and incentives for conservatism. Academic research has the opportunity to suggest fresh approaches to difficult problems. That is our aim and, in so doing, we also contribute to the body of theoretical work that has explored the interactions of the three players, auditors, managers, and shareholders, and how the interactions are affected by different incentive schemes.

In the following section, we review the relevant theoretical and empirical literature. In Section III, we develop a mathematical model of the interactions between the two principals—management and the audit committee—and their agent, the auditor. First, we examine a regime in which audit fees cannot be contingent on outcome, but managers can influence auditors with consulting contracts. We follow this with an analysis of a regime allowing contingent audit fees. We finish Section III with a numerical example, characterizing the equilibrium strategies of managers and the audit committee for a fixed set of parameters. In Section IV, we look at the implications of a multiperiod world, and in Section V, we conclude.

II. RELATED LITERATURE

Common agency, first modeled by Bernheim and Whinston (1986), extends the principal-agent model to address situations in which multiple principals contract noncooperatively with a single agent. In their model, multiple principals announce incentive schemes for the agent whose action is not observable by the principals. In our model, the agent makes a decision that is publicly learned at the time of contracting, but the underlying information is not. Recent work on common agency includes theoretical extensions of the framework (e.g., Laussel and Le Breton 2001; Peters 2001) as well as applications of models with a multiprincipal structure (e.g., Dixit et al. 1997). Our work falls in the latter category.

We use the common agency framework to model the relationship between the management, owners, and auditor of a firm. Our work is related to Antle (1984), which also looks at the incentives and actions of these three parties, with a different configuration. Antle (1984) models the auditor and manager as effort-averse agents and evaluates the compensation contracts offered to them by owners. That paper considers auditors that (1) are strongly independent (i.e., work in the interest of shareholders), (2) are independent (i.e., self-interested but unable to enter into collusive arrangements with management), and

(3) lack independence (i.e., collude with managers via side payments). In Antle's (1984) model, owners offer payment schedules to the auditor and the manager, then the auditor and manager respond by choosing "effort" levels and reporting strategies. The owners are always better off with independent auditors, but for any incentive plan, auditors are at least as well off forgoing independence as maintaining it. Using Antle's (1984) classification, the auditors in our model lack independence in that they are self-interested profit maximizers who can be influenced by management. Managers can contract with auditors, but use of the common agency framework permits the audit committee to recognize the incentives offered by the manager and undo them. This in turn realigns auditors' interests with shareholders' interests for truthful reporting.

Antle and Demski (1991) focus on the economies of scope that arise when an incumbent auditor has superior information about the cost of consulting services. In their model, a client firm chooses the levels of audit services and consulting services from both an incumbent audit firm and a replacement firm. Contracting frictions, such as minimum audit quantity requirements or restrictions on the fee schedule, can further enhance the economies of scope between auditing and consulting. However, they omit an examination of auditor independence and misreporting. As in Antle and Demski (1991), we model incumbent auditors as better at providing nonaudit services due to economies of scope. While we do include a notion of competition for provision of nonaudit services, absent from our model is the competition for basic audit services.

Antle and Nalebuff (1991) explicitly model the auditor-client negotiation process that leads up to the client's release of the financial statements and the auditor's opinion of them. They model the auditor and the audit committee as utility-maximizing entities who care about the possible discrepancies between actual and reported earnings, as we do in this paper. The auditor makes an offer of an earnings report and the client either accepts (in which case the audit is done) or rejects (in which case they share the cost of an extended audit). The authors argue that although the auditor may start with a conservative offer, that does "not necessarily lead to a conservative final report" (Antle and Nalebuff 1991, 33). They analyze the interaction between an owner/manager (as a single entity) and the auditor, and we explicitly analyze the effects of divergent interests of the owners (shareholders), managers, and auditors.

Recently, several empirical papers have examined whether the joint provision of audit and nonaudit services impairs auditor independence. Frankel et al. (2002) find a positive association between nonaudit fees and earnings management, as measured by proximity to earnings benchmarks and discretionary accruals. In contrast, Francis and Ke (2002) do not find higher nonaudit service fees associated with meeting or beating earnings targets. Given the mixed evidence, we capture the relation between nonaudit fees and earnings management by modeling the optimal level of consulting (or nonaudit services) based on the costs and benefits of those services. Thus, it is not the level of nonaudit service fees per se that may drive earnings management, but rather the excess fees. Put differently, earnings management is related to nonaudit services fees only after conditioning on the efficiency differential between auditors and outside providers for nonaudit services.

Some have argued that auditor independence or freedom from bias is psychologically impossible, given (1) the close working relationships of auditors and their clients and (2) the fact that the clients pay their own auditors (Bazerman et al. 1997). However, King (2002) finds that the psychological forces associated with group affiliation (e.g., engagement teams, professional organizations) can neutralize the bias that arises from management's ability to persuade auditors of their honesty through personal relationships. We do not address the psychological or ethical questions regarding truth-telling; our auditors are modeled as rational agents who respond to economic incentives.

III. THE MODEL

> Of fundamental importance in understanding the conflict of interest that arises from the provision of nonaudit services to audit clients is the fact that in so doing the audit firm is really serving two different sets of clients: management in the case of management consulting services and the audit committee, shareholders and all those who rely on the audited financials and the firm's opinion in deciding whether to invest, in the case of the audit...It is obvious that in serving these different clients the firm is subject to conflicts of interest that tear at the fragile fabric of loyalty owed to one client or the other. (Panel on Audit Effectiveness 2000, 108)

Based on the above quotation from the Panel on Audit Effectiveness, we model the auditor-client-manager relationship as a common agency. Two different, risk-neutral parties (management, *m*, and the shareholders, *s*, represented by the audit committee) use the services of a single audit firm. In this section, we develop a model of these parties' actions and compare the outcomes in regimes in which audit fees are allowed to be contingent to ones in which they are not allowed to be contingent.

We briefly outline the sequence of events here and develop the notation and details below. We begin at the point at which the audit committee engages the audit firm to conduct their required audit. The auditor then performs the audit, during which he learns information about the performance of the firm. After the audit work, the manager proposes an earnings report and offers a nonaudit service contract to the auditor. The auditor can accept the report and the offer for nonaudit services work, or reject them. The firm issues financial statements with management's report and the auditor's opinion. The opinion can be unqualified, indicating acceptance of the report, or qualified, indicating rejection of report. The auditor is then compensated for the audit. Finally, the auditor completes the nonaudit services (if any) and is paid by the manager for these services. At a later date, if the current period's actual and reported earnings differ, reported earnings are restated. The events are depicted in Figure 1, the notation is summarized in Table 1, and we discuss the details of the model below.

FIGURE 1
Timeline of Events

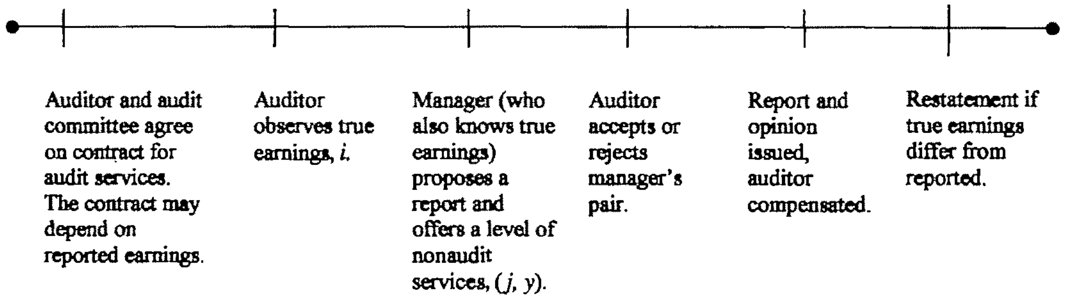
Auditor and audit committee agree on contract for audit services. The contract may depend on reported earnings.
Auditor observes true earnings, *i*.
Manager (who also knows true earnings) proposes a report and offers a level of nonaudit services, (*j*, *y*).
Auditor accepts or rejects manager's pair.
Report and opinion issued, auditor compensated.
Restatement if true earnings differ from reported.

TABLE 1
Table of Notation

N = number of states;
i = level of true earnings;
j = level of reported earnings;
y^s = vector of payments for audit services;
y_j^s = audit service payment for report j;
$Q^s(j - i)$ = cost for shareholders of report j when true earnings are i;
$v_a(x)$ = marginal benefit of hour x of non-audit services;
z = number of bonus states;
b_j = manager's bonus in state j;
y_j^m = nonaudit service contract offered by the manager to the auditor with report j;
α = manager's sensitivity to auditor's compensation;
$Q^a(j - i)$ = error cost for auditor;
K = conservatism parameter;
$A_i(j_i, y_{ji}^m, y_{ji}^s)$ = auditor's decision rule (accept or reject); and
D_j = manager's maximum willingness to pay for a report of j.

Background

We model the possible levels of earnings i as a discrete set $i \in \{1,...,N\}$, where 1 is the lowest level of earnings and N is the highest. We assume that the report of earnings, j, also comes from this set. By this restriction, we try to capture the notion that auditors do not permit out-and-out fraud, and will only issue unqualified opinions for earnings levels within what they perceive to be a defensible range. Implicitly, we assume there is sufficient flexibility in accounting standards to allow the auditor to find some evidence supporting a level of earnings different from his private information.[4]

Stage 1: Contracting and Performing Audit

One of the primary functions of audit committees is to retain and supervise the audits of their firms. In this role, the audit committee contracts with the auditor for the audit. Currently, professional conduct codes for auditors prohibit contingent contracts, that is, payment schemes that depend on the outcomes of the audit. In this paper, we investigate how a relaxation of this restriction can improve truthful reporting.

To this end, we model the payment from the audit committee to the auditor as a vector y^s with the elements y_j^s. When we discuss the special case of noncontingent fees, the y_j^s are equal for all reports j. After the contract y^s has been established, the auditor conducts the audit work. During this time, the auditor learns the earnings level i of the firm.[5] In this paper, we do not address the moral hazard questions: Will the auditor work hard? How much work is needed for a good audit? This allows us to focus on an issue central to auditor independence: How can managers' incentives to influence auditors be mitigated by actions taken by the audit committee?

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[4] The recently passed Sarbanes-Oxley Act reiterates the need for direct communication between auditors and audit committees. Still, in general, the audit committees will not know *all* of the auditor's private information.

[5] For simplification, we assume there is a "true" level of earnings. However, an alternative interpretation is that the auditor observes a representative sample of accounts from which he could make an unbiased estimate of income.

Given the existence of many investment opportunities, shareholders prefer accurate reporting information so they can allocate their capital to firms with the best expected performance. The audit committee, which represents shareholders, thus prefers truthful reports. To quantify the loss shareholders realize from untruthful earnings reports, we assign the shareholders a loss function, $Q^s(j - i)$, which depends on the difference between reported earnings j and actual earnings i. Of course, the shareholders do not know i *ex ante*. The preference for truthful reports implies that the function $Q^s(j - i)$ has its minimum value at $i = j$; we let $Q^s(0) = 0$. We assume $Q^s(j - i)$ is convex in its argument (e.g., a quadratic form) to capture the increasing marginal costs as reports deviate more and more from the truth.

The audit committee wishes to minimize the expected payments to the auditor and costs to shareholders of inaccuracies in the financial statements. Details of the audit committee's objective function are given later.

Stage 2: Manager's Proposal

The manager's proposal to the auditor has two parts: a level of earnings and an accompanying contract for nonaudit services. Like the auditor, the manager knows true earnings i. Based on this knowledge, the manager chooses a reporting level j. Where appropriate, we refer to the proposed reported level j given the true level of i as j_i. With the proposed earnings report j, the manager also offers the auditor a nonaudit services contract.

Antle and Demski (1991) discuss the synergies between audit and nonaudit services that allow audit firms to perform nonaudit services more efficiently than outsiders. We capture the economies of scope between audit and nonaudit services by assuming that the auditor can provide a higher level of benefit per consulting hour than can an outside consultant. Let $v_a(x)$ be the function describing the marginal benefit of hour x of consulting services provided by the auditor, and let $v_o(x)$ be the marginal benefit from an outside consultant, with (1) $v_a'(x) < 0$ and $v_a(x) > 0$, and (2) $v_o'(x) < 0$ and $v_o(x) > 0$. Due to the synergies of providing both types of services, the auditor is more efficient, or $v_a(x) > v_o(x)$ $\forall$ x. The level of consulting services the manager would like to purchase from either the auditor x_a^* or an outside consultant x_o^* equates the marginal benefit of the services with the marginal cost of acquiring those services. If the billing rates of the auditor and the competitor are equal, then because the auditor is more efficient, $x_a^* > x_o^*$.[6] These relationships are shown in Figure 2.

Given reported earnings are above a threshold level, we assume the manager's bonus is increasing in the level of reported earnings, where b_j is the bonus when earnings are reported to be j. Below the threshold, there is no bonus. There are $z < N$ states with nonzero bonuses, or equivalently, the threshold is $N - z$. If the manager can gain an additional bonus by persuading the auditor to agree to liberally reported earnings, then he may be willing to expend some additional resources on excess consulting. That is, if the auditor agrees to a favorable earnings report, then there exists $x_a' > x_a^*$ such that the manager is better off purchasing x_a' (hours of) consulting services from the auditor and reporting high earnings than either x_a^* from the auditor or x_o^* from the outsider and reporting low earnings. In this scheme, the x_a' can depend on the reported earnings level j. Rather than specify a functional form for $v_a(x)$, we work directly with the manager's net cost for excess nonaudit services when earnings level j is reported, denoted y_j^m. We define:

---

[6] Observing the joint provision of audit and nonaudit services from a single audit firm would not be evidence of impaired independence. It is only the misuse of nonaudit services, through excess fees, that leads to independence problems.

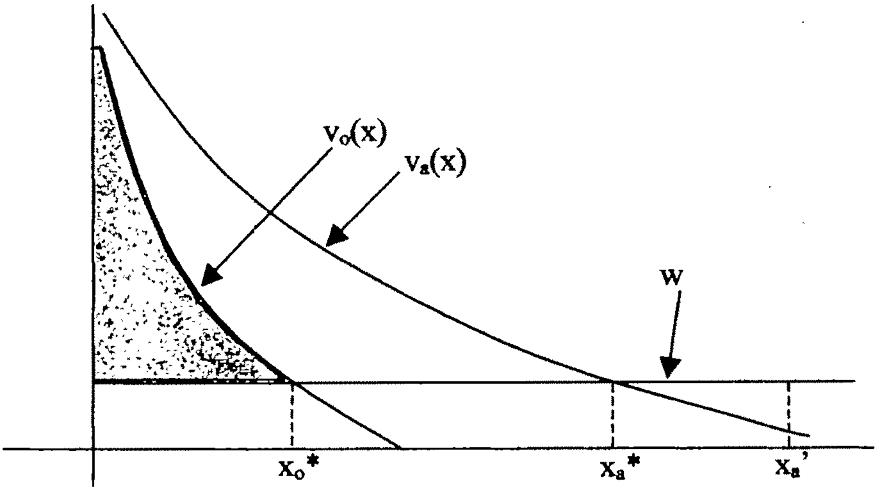
$$y_j^m = (x'_{aj} - x_a^*)w - \int_{x_a^*}^{x'_{aj}} v_a(x)\,dx$$

where w = auditor's hourly wage.

The manager's objective is to maximize his bonus less the (net) cost of the nonaudit

FIGURE 2
Marginal Costs and Benefits of Nonaudit Services from Auditor and Outside Consultant

Panel A: Shaded area is the total benefit to firm purchasing optimal amount of consulting services from outsider (i.e., not the auditor).



Panel B: Shaded area is the total benefit to firm purchasing optimal amount of consulting services from the auditor.

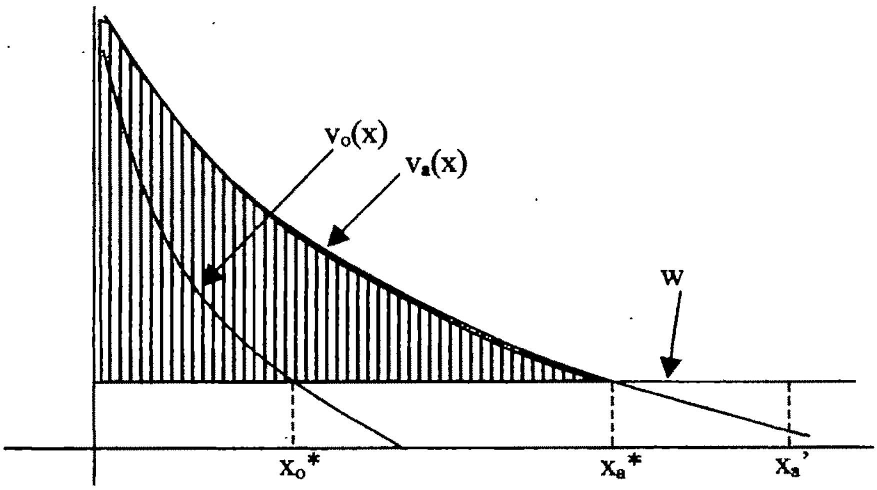


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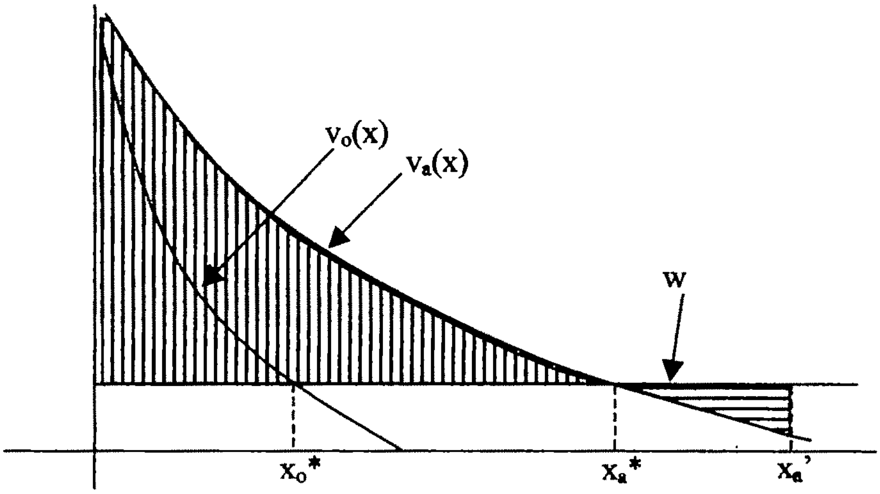
FIGURE 2 (continued)

Panel C: The difference in areas between the vertical shaded area and the horizontal shaded area is the total benefit to firm purchasing more than the optimal amount of consulting services from the auditor.



service contract, subject to the existing contract between the auditor and the audit committee. We assume the manager values the net cost (costs less benefits) of services from the audit firm at a rate less than the rate at which he values his bonus. Therefore, the manager is less sensitive to the auditor's compensation costs than is the audit committee. The manager may use the resources not spent on compensating the auditor to consume perquisites, but at a rate of α, where $\alpha \in (0,1]$ is known by all players. The expression for the manager's objective function is given below, after the notation for the auditor's strategy is introduced.

Stage 3: Auditor's Opinion

The auditor accepts or rejects the package (j, y_j^m).[7] If he accepts, then he issues an unqualified opinion on the financial statements and signs the contract for performing the nonaudit services. If he rejects, then he issues a qualified opinion on the financial statements and does not sign the contract for the nonaudit services. We assume that shareholders interpret a qualified opinion negatively.[8] Specifically, we assume that users of financial reports infer the lowest level of earnings in the defensible range, $j = 1$, after a qualified opinion is issued, and compensate the auditor as such.

The auditor's decision to accept or reject, given his knowledge of true earnings i, the y^s audit contract, and the manager's proposal $(j_i, y_{j_i}^m)$ is based on the payments he will receive as well as the expected cost of legal liability from misreporting. We model the expected legal liability as a function of reported and true earnings $Q^a(j - i)$. Based on the historical patterns of litigation, it is easier to demonstrate damages (losses) from actual rather than hypothetical investments. This asymmetry contributes to the culture of conservatism among auditors. Belkaoui (1985) captures the spirit of it by saying "[t]he principle implies that preferably the lowest values of assets and revenues and the highest values of liabilities and expenses should be reported." Consequently, we assume that auditors have a preference for conservatism (i.e., they prefer to underreport rather than overreport earnings). Arnold and Edwards (1993) also document conservatism in a case study involving going-concern evaluations. To capture the idea of conservatism, we let $Q^a(j - i) = -K$ for $j < i$. For $j \geq i$, we do not restrict the form of $Q^a(j - i)$ except to require that $Q^a(j - i)$ is convex over the entire range of its argument, with $Q^a(0) = 0$.[9] This captures both the asymmetry between conservative and liberal reporting, with the auditors preferring conservative reporting, as well as the increasing marginal cost of accepting inflated earnings reports.

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[7] Unlike Antle and Nalebuff (1991), the auditor and manager do not have ongoing negotiations about the report. There is a single offer made, which the auditor accepts or rejects.

[8] This assumption is consistent with Dopuch et al. (1997), who find negative abnormal stock returns associated with the disclosure of qualified audit opinions.

The utility-maximizing auditor will accept the manager's proposal if and only if the net benefit of accepting report j exceeds the net benefit of rejecting the report (where the auditor is compensated as if the report were $j = 1$), or:

$$A_i(j_i, y^m_{j_i}, y^s_{j_i}) = \begin{cases} 1 & \text{if} \quad y^s_{j_i} - y^s_1 + y^m_{j_i} - y^m_1 \geq Q^a(j - i) - Q^a(1 - i) \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

where $A_i(j_i, y^m_{j_i}, y^s_{j_i}) = 1$ for an acceptance, and 0 for a rejection.

With this notation for the auditor's strategy, we can express the audit committee's objective function from stage 1 and the manager's objective function from stage 2 as follows. The audit committee's objective function is:

$$\min_{y^s} \sum_i p_i[(Q^s(j^*_i - i) + y^s_{j^*_i})A^*_i + (Q^s(1 - i) + y^s_1)(1 - A^*_i)] \tag{2}$$

where p_i is the *ex ante* probability of earnings level i, j^*_i is the report the manager offers in earnings state i, and A^*_i is the indicator variable for the auditor's optimal accept (1) or reject (0) decision.

The manager's objective function given an observed level of earnings i and the audit contract y^s:

$$\max_{y^m_j, j}(b_j - \alpha y^m_j)A^*_i \tag{3}$$

Stage 4: Follow Through

In the final stage of the process, the financial statements are publicly issued, the auditor is paid for the audit, and the nonaudit services contract is fulfilled. If this period's actual and reported earnings differ, then reported earnings are later restated.

Contingent Audit Fees Disallowed

Rule 302 of the AICPA Code of Conduct prohibits contingent fees for audit services (i.e., fees based on the outcome of the audit). The purpose of this rule was to help maintain objectivity and independence. "Contingent fees result in the auditor having a mutual interest with audit client in the outcome of the work performed" (Securities and Exchange Commission 2000). This prohibition eliminates some obvious independence issues, such as the one in which the firm pays the auditors more for favorable (high earnings) reports than for unfavorable reports. An auditor's legal liability disciplines the auditor from allowing excessively favorable reports when facing a flat fee. In this section, we restrict the audit committee to use contracts with a fixed fee, which we normalize to zero. That is, for any level of reported earnings, the auditor will receive the same level of compensation for his audit services.

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[9] The numerical example at the end of this section contains an example of a functional form that meets these requirements.

As a benchmark case, suppose the manager cannot compensate the auditor for nonaudit services, implying $y_j^m = 0$ for all reports j. In this case, neither principal has the ability to provide incentives to affect the auditor's strategy. As such, the auditor will choose the strategy that maximizes his own utility. Given the auditor's preferences for conservatism, he will accept any report that understates observed earnings.

Now we consider the case in which management can provide incentives over the strategies of the auditor but the audit committee cannot. We allow the manager to award nonaudit service contracts that *de facto* provide contingent compensation.[10] More specifically, the manager creates a payment scheme (corresponding to the award of nonaudit services), based on reported earnings knowing the auditor will use his preferred strategy.[11]

Proposition 1: For any level of earnings i observed, the auditor will accept the pair (N,y) if $y \geq Q^a(N-1)$.

Proof: All proofs are in the Appendix.

The nonaudit service contract offered in Proposition 1 is the lowest cost way for the managers to induce the most egregiously impaired independence. In the next proposition, we examine the conditions under which the manager would pursue such a strategy. The manager balances the costs of compensating the auditor with the benefits of doing so. Proposition 1 focuses on the costs in isolation while Proposition 2 trades off the benefits to the manager (i.e., his bonus) with the required expenditure.

Proposition 2: For any level of earnings i observed, the manager offers the pair $(N,Q^a(N-1))$ if $b_N - b_j \geq \alpha(Q^a(N-1) - Q^a(j-1))$ for all j.

The condition on this proposition implies that the manager's bonuses are increasing significantly with an increased earnings report. The significance is measured relative to the cost to the auditor of agreeing to an exaggerated report, $Q^a(j-1)$. For the auditor to be indifferent between accepting a truthful or a conservative report and an alternate report, regardless of his private information, his compensation for accepting a report of j must offset his highest misreporting cost. This cost would be incurred when true earnings are the lowest ($i = 1$) and reported earnings are the highest ($j = N$). Propositions 1 and 2 highlight the significant influence the manager will have over reporting if he can compensate the auditor via nonaudit services and the audit committee is constrained to fixed audit fees. It is this concern, that auditors may be influenced by excessive *nonaudit* service fees, that has prompted the SEC to restrict the provision of nonaudit services to audit clients for certain types of nonaudit services.

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[10] Conceivably, the manager could compensate the auditor for nonaudit services based on the combination of actual earnings and reported earnings. However, we disallow it in our model.

[11] Such contracts seem consistent with the empirical findings in Frankel et al. (2202) that higher nonaudit service fees are associated with greater earnings management.

Contingent Audit Fees Allowed

To alleviate the independence problem that arises when managers use nonaudit service contracts to persuade auditors to use a particular strategy, the SEC has imposed restrictions on auditors' joint provision of audit and nonaudit services. While this restriction might cut down on the use of nonaudit services to influence auditors, it ignores the possible synergies achieved by using the same firm for both types of services (Antle and Demski 1991). Moreover, it may not necessarily increase the truthfulness of reports, if the auditor's preference is for conservatism.

This section considers how ***deregulation*** of audit fees can allow for more truthful reporting. If audit fees can be contingent on reported earnings, then the audit committee can determine a payment vector y^s that will "undo" the incentives provided by the manager. Through contingent fees, the audit committee will recover truthful reporting for some parameters. Then, we have the following proposition:

> **Proposition 3:** When contingent audit fees are permitted, if $\min\{Q^s(1), Q^s(-1)\} \geq K$, then a truthful reporting equilibrium exists.

Recall that b_j is the manager's bonus paid when reported earnings are j. Let $D_j = b_j/\alpha$. When $D_N \geq K$, the audit committee offers payments (in the truthful reporting equilibrium) of:

$$y_j^s = \begin{cases} D_N - K & j = 1 \\ D_N & j \in \{2, \ldots, N - z\} \\ D_N - D_j & j > N - z \end{cases} \tag{4}$$

Notice that the audit committee's payments first increase, then decrease. The intuition for this is simple. *Ceteris paribus*, the auditor has a preference for rejecting any report that is not conservative. In the bonus states, the manager would be willing to compensate the auditor for accepting the nonconservative reports. On the other hand, the manager would never compensate the auditor (via nonaudit services) for a report in a non-bonus state (i.e., where his bonus is zero), and it is precisely in that region that audit committees must provide the incentives.

Given the payments from the audit committee above, the manager will offer:

$$(j, y) = \begin{cases} (i, 0) & i < N - z + 1 \\ (i, D_i) & \textit{otherwise} \end{cases}$$

Off equilibrium (i.e., if the audit contract is different from that described by Equation (4)), the manager will select the pair that maximizes Equation (3). The manager is providing a significant portion of the incentives to accept truthful reports in bonus states. The audit committee, by providing strong incentives to the auditors in the non-bonus states, cuts off the manager's ability to get overreporting and, thus, "undeserved" bonuses.

Given the payments of Equation (4) and the offered pair described in the previous paragraph, the auditor will accept the contracts and report. Any unexpected offers (off equilibrium) will be evaluated using Equation (1). Because the audit committee and the manager know the auditor's decision rule, they prefer to offer the contracts and reports given above.

If instead $D_N < K$, the audit committee offers payments of:

$$y_j^s = \begin{cases} 0 & j = 1 \\ K & j \in \{2,\ldots,N - z\} \\ K - D_j & j > N - z \end{cases}$$

As before, the manager and auditor maximize their net benefits. This case is somewhat less interesting, in that independence would not be as severely impaired as in the first case. That is, the amounts that the managers would have to pay the auditors to overreport exceed the benefits (higher bonuses), even if audit committees could not pay contingently.

Several points regarding Proposition 3 are worth noting. The equilibrium payments are different in the two regions $D_N \geq K$ and $D_N < K$ to ensure that the audit committee makes non-negative payments in all states. Because the audit committee represents the overall body of users of financial statements, it is reasonable to imagine that they would have the highest costs of misreporting ($Q^s(1) > K$ and $Q^s(-1) > K$), so a contingent-fee regime has the potential to increase investors' welfare. The audit committee effectively *undoes* the incentives provided by the manager via compensation for lower level earnings reports. If the auditor and the shareholders' preferences were perfectly aligned (i.e., the auditor prefers truthful reporting), there would be no pure strategy equilibrium.[12]

Figure 3 graphically depicts the equilibrium compensation vectors determined by Proposition 3. In Panel A, when the manager has the greatest ability to influence the auditor, the manager offers payments that are increasing in his bonus (i.e., higher consulting fees for high earnings levels) and the audit committee's payments start low, increase, and then decline (assuming the bonus payments are strictly increasing). The net compensation is weakly increasing in reported earnings, but only enough to get the auditor to accept a truthful report, not enough to get the auditor to accept overreporting. In Panel B, where the cost of motivating the auditor to accept a report j equal to N, independent of his private information, is too high for the manager, the audit committee must make more of the total contribution to get the auditor to move away from their preference toward conservatism.

A Numerical Example with $N = 4, z = 2$

We use the parameters given by Table 2. If the auditor is not compensated for excess nonaudit services, then he will only accept conservative earnings reports. The shareholders incur high error costs, as there is less information in audited earnings. Now suppose the manager offers excess nonaudit services and an earnings report of $j = 4$. In the case of noncontingent audit fees, the condition from Proposition 2 is satisfied ($b_N - b_j \geq \alpha(Q^a(N - 1) - Q^a(j - 1))$ for all j) so the manager offers the pair (4, 300), and the auditor will accept the report/nonaudit service pair for every realization of true earnings. The auditor receives a fixed payment from the audit committee. If he accepts the report, then he will earn 300 for excess nonaudit services, and if he rejects the report, he will not perform those additional nonaudit services for the firm.

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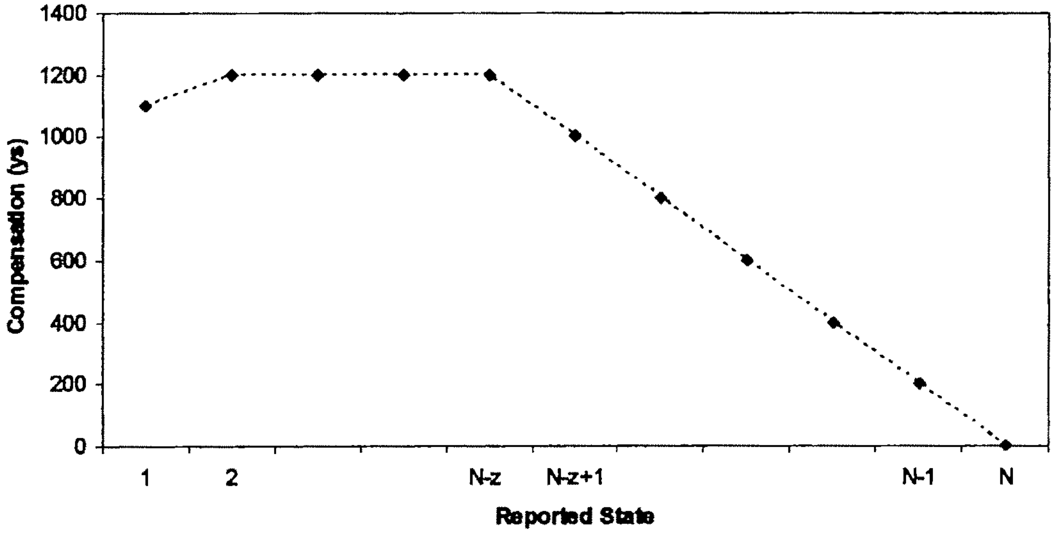
[12] Suppose the manager offers compensation to move the auditor away from the truth. In turn, the audit committee responds by offering compensation to reinstate the truth. The manager will certainly want to change his strategy, since his payments are higher than they were initially, and he is not getting any additional bonus. For this reason, this paper focuses on relaxing the restriction on contingent fees, rather than the rule that prohibits auditors from holding shares in their clients' stock (i.e., an action that would more closely align auditor and shareholder interests). This argument rules out $K = 0$ in our model.

FIGURE 3
Shape of Audit Committee and Management Payments in Equilibrium

Panel A: Audit Committee and Manager Payments to Auditor if $D_N \geq K$

Audit committee's payments



Manager's payments

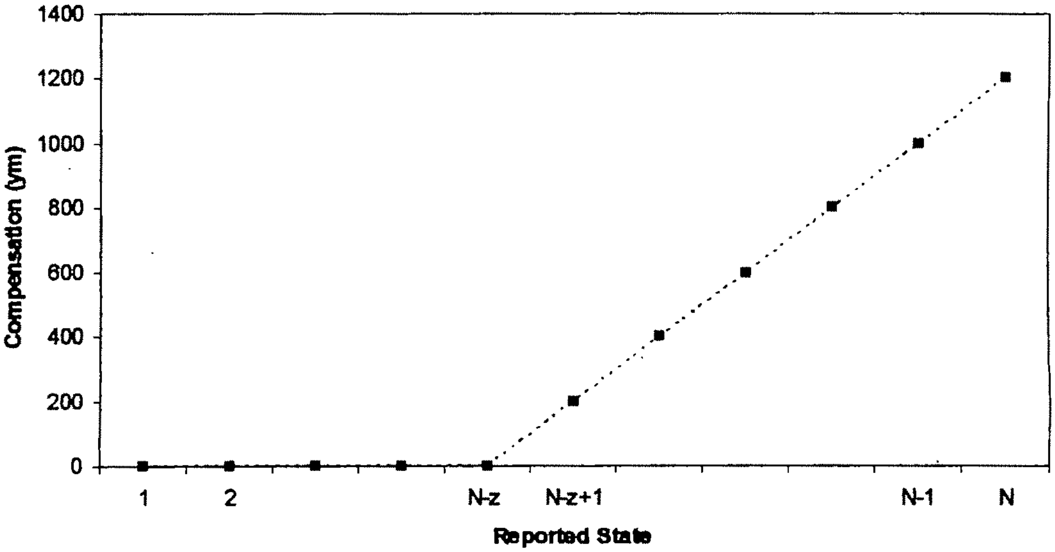


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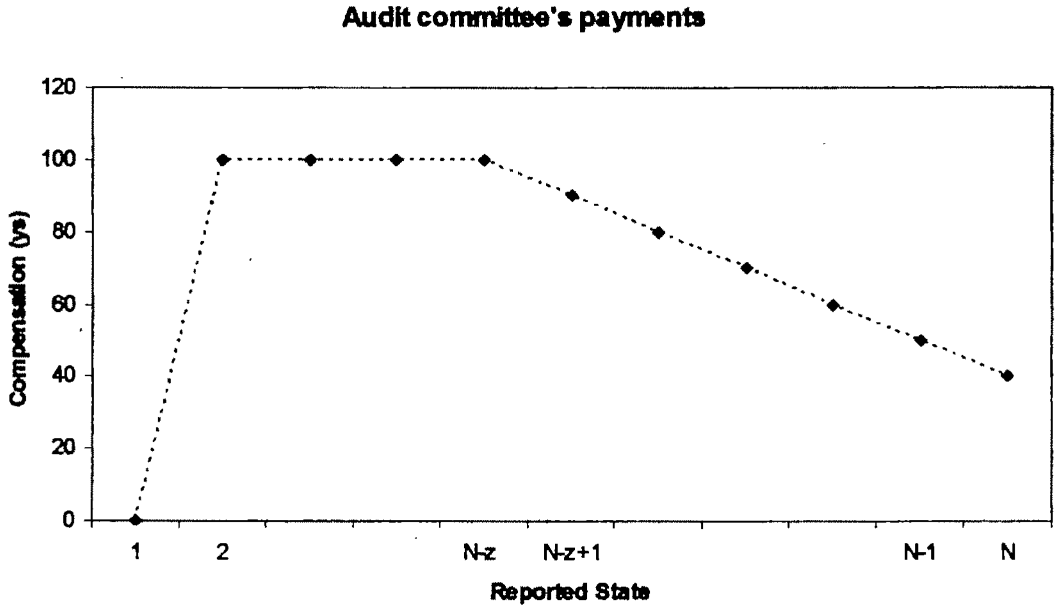
FIGURE 3 (continued)

Panel B: Audit Committee and Manager Payments to Auditor if $D_N < K$



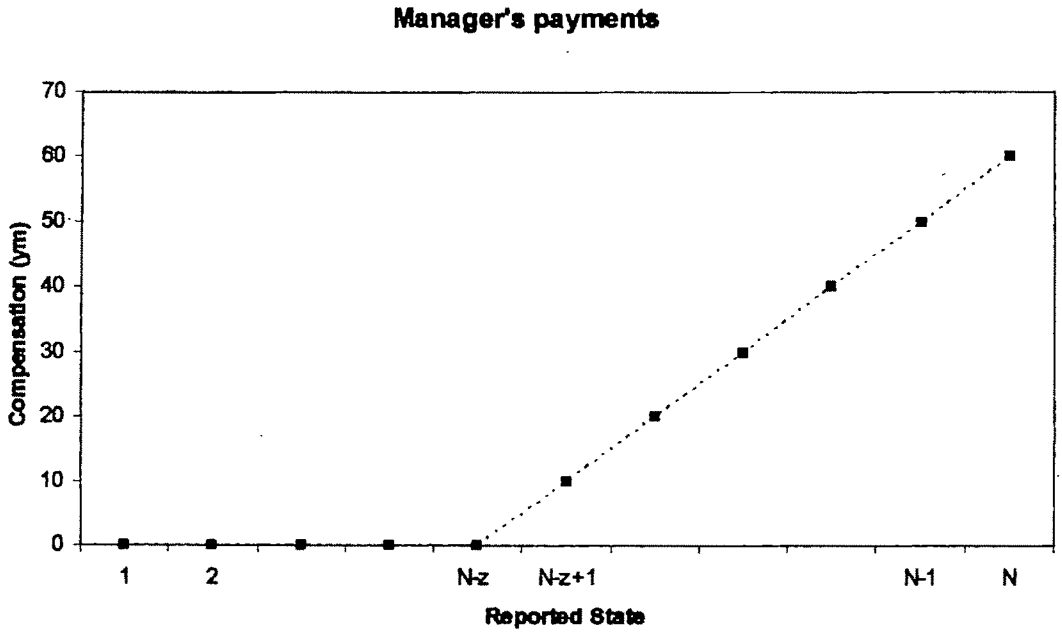


TABLE 2
Parameters for Numerical Example

N = number of states;	4
z = number of bonus states;	2
b_j = manager's bonus in state j;	$2(j-2)$
a = manager's compensation sensitivity;	0.01
$Q^a(j-i)$ = error cost for Auditor;	$\begin{cases}(j-i)K & j \geq i-1 \\ -K & j < i-1\end{cases}$
K = from auditor cost function; and	100
$Q^s(j-i)$ = error cost for shareholder:	$350(j-i)^2$

Here, reported earnings are completely uninformative, since the earnings level $j = 4$ is always reported and accepted, and the manager gets a bonus regardless of the true state.

Turning to the case in which audit fees can be contingent on reported earnings, the manager offers (1, 0) when true earnings $i = 1$, (2, 0) when $i = 2$, (3, 200) when $i = 3$, and (4, 400) when $i = 4$. The audit committee offers, for reported states 1 through 4, $y^s = [300, 400, 200, 0]$.

If the auditor accepts the manager's report pair, then his costs are zero (since the report is truthful) and his benefits are 300 when the true earnings level is 1, and 400 otherwise. Alternatively, the auditor could reject the report, and issue a qualified opinion, whereby he is rewarded as if the reported earnings level is 1. Consider the case when the true earnings level is 2. If the auditor accepts the pair (2, 0), then he receives 400 in compensation from the audit committee, and incurs zero costs (as the report is truthful). On the other hand, if he rejects, then he receives 300, and gets the "conservatism" benefit of 100, for a total of 400. Since he is no better off than accepting the report, he will not reject (qualify the report). An identical argument follows for all other earnings levels and offered pairs.

IV. EXTENDING THE MODEL TO A MULTIPERIOD FRAMEWORK

One of the most salient features of the auditor-client relationship is its ongoing nature. There are incentives for both the auditor and the client to maintain the relationship. In particular, the client's decision to switch auditors is viewed unfavorably by market participants.[13] In this section, we extend our model to incorporate multiple periods. Under the assumption that a firm is a going concern, we consider an infinite horizon model. In each period, the manager makes an offer and the auditor is faced with an accept/reject decision. Although the values of the earnings levels might shift over time, we assume that in every period there are N possible levels for earnings and that the manager is paid a bonus in the top z states.

The audit committee makes a binding recommendation regarding the selection or replacement of the independent auditor after observing the most recent earnings report and opinion. If the audit committee replaces the current auditor, then there are at least two rival auditors (of equal quality) who can conduct future audits. The rival auditors have no prior relationship with this client. The incumbent auditor has a cost advantage in providing audit services since he is familiar with the company's operations, procedures, and the industry setting and has developed efficiencies through prior audit engagements. Thus, if the incumbent auditor is fired, then the firm will incur a switching cost of g, which can be loosely interpreted as the total of (1) search costs, (2) the costs to familiarize the rival auditor, and (3) the increased probability of problems that arise due to lack of experience with the firm.[14] We let q_j be the probability that the incumbent auditor is retained by the audit committee when the reported earnings are j. Figure 4 provides the timeline for the multiperiod game.

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[13] For example, Fried and Schiff (1981) document a negative market reaction to auditor switches.

Contingent Audit Fees Disallowed

In the multiperiod setting, the auditor's decision is now a function of both current and future expected compensation.[15] In this section, we assume the audit committee cannot pay the auditor contingently, but does have the ability to make retention (or replacement) decisions. The audit committee, as seen in Figure 4, commits to the conditional retention probabilities prior to the report. If the auditor believes that the contingent portion of his total income depends solely on managerial decisions, then he may be willing to accept overstated reports to maintain access to current and future consulting revenues. However, the threat of dismissal by the audit committee can effectively discipline the auditor.

If all N states are equally likely, then the auditor's present value of accepting a truthful report with the associated nonaudit services can be expressed recursively as:

$$V = \frac{1}{N}\sum_{i=1}^{N} y_i^s + y_i^m + q_i \beta V \tag{5}$$

where V is the value of the total engagement and $\beta \in (0,1)$ is the one-period discount factor.

Using the fact that the audit committee pays a fixed compensation y_*^s and rearranging we have:

$$V = \frac{1}{N}\left(Ny_*^s + \sum_{i=1}^{N} y_i^m + \sum_{i=1}^{N} q_i \beta V\right); \tag{6}$$

$$V = \frac{Ny_*^s + \sum_{i=1}^{N} y_i^m}{N - \beta \sum_{i=1}^{N} q_i}. \tag{7}$$

The value V is increasing in total current compensation, the retention rates, and the discount factor. The audit committee must then determine cost-minimizing compensation and retention levels that motivate the auditor to accept only truthful reports in every period.

The audit committee uses the following program for a truth-telling equilibrium:

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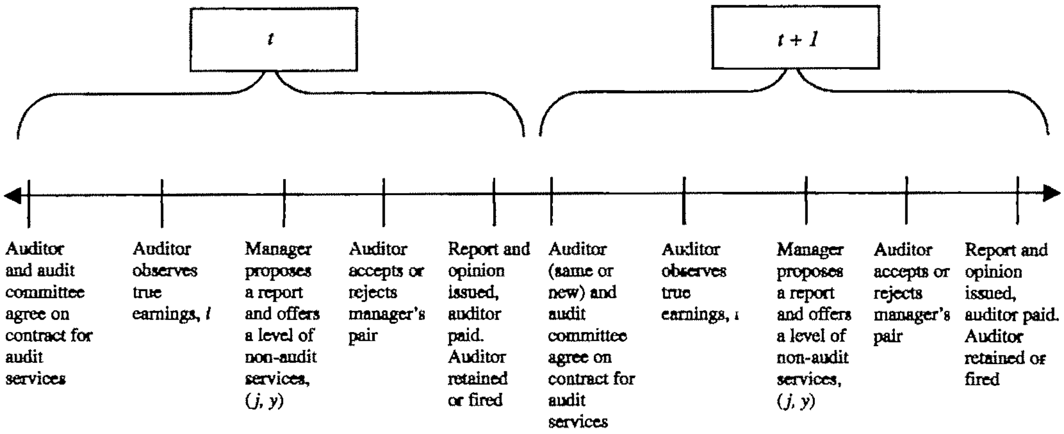
[14] The Quality Control Inquiry Committee of the SEC Practice Section showed that allegations of audit failure occur almost three times as often when the audit firm is performing its first or second audit of the company.

[15] DeAngelo (1981) discusses the problem of independence in a multiperiod framework, concluding that it is not "low balling" in the first period that impairs auditor independence, but rather the auditor's possibility of gaining rents in subsequent periods.

FIGURE 4
Timeline of Events in Multiperiod Setting



t
t + 1
Auditor and audit committee agree on contract for audit services
Auditor observes true earnings, i
Manager proposes a report and offers a level of non-audit services, (j, y)
Auditor accepts or rejects manager's pair
Report and opinion issued, auditor paid. Auditor retained or fired
Auditor (same or new) and audit committee agree on contract for audit services
Auditor observes true earnings, i
Manager proposes a report and offers a level of non-audit services, (j, y)
Auditor accepts or rejects manager's pair
Report and opinion issued, auditor paid. Auditor retained or fired

$$\min_{y_*^s, q_i} y_*^s + \frac{g}{N}\sum_{i=1}^{N}(1 - q_i)$$

s.t.

$$y_j^m + y_*^s + q_j\beta V \geq y_1^m + y_*^s + q_1\beta V + K \qquad j \in \{2,\dots,N\}. \qquad (8)$$

$$y_{j'}^m + y_*^s + q_{j'}\beta V - Q^a(1) \leq y_1^m + y_*^s + q_1\beta V + K \qquad j' \in \{2,\dots,N\}$$

$$y_2^m + y_*^s + q_2\beta V - Q^a(1) \leq y_1^m + y_*^s + q_1\beta V$$

Recall that g is the cost of replacing the current auditor. The first set of constraints ensures that accepting truthful reports is more attractive to the auditor than rejecting them. The constraint in the second line ensures that the auditor will reject overstated reports. The minimum cost of overstatement is $Q^a(1)$, so we use this on the left-hand side because it is the most restrictive. Finally, the last constraint is for the special case of true earnings $i = 1$, where a rejection is not "conservative."

Proposition 4: In a truth-telling equilibrium, the probability of retaining the auditor increases from state 1 to state 2, is constant for non-bonus states 2 through $N - z$ and is decreasing in the bonus states $N - z + 1$ to N.

The probability of firing the auditor increases the higher the reported earnings, conditional on being in a bonus state. If reported earnings are not in a bonus state, then the probability of firing the auditor is fairly low, except when reported earnings are as low as possible (i.e., interpreted to be level 1, in the case of a qualified opinion). Intuitively, the auditor needs to be "punished" for rejecting nonconservative earnings (his natural preference) or accepting very high earnings (for which the manager has provided him incentives). Since the audit committee is restricted to a fixed payment, it mimics the single-period-contingent-fee case through penalties for reports. In Proposition 3, the audit committee's payments increased from state 1 to 2, then flattened, and finally decreased from $N - z$ to N. Here the retention probabilities follow exactly the same pattern. The audit committee is able to mimic contingent fees, and achieve a truthful reporting environment.

This result hinges on the fact that in every period, there is a positive probability of continuing the auditor-client relationship. If auditor rotation were mandatory, then the players would face the same problem as the single-period analysis in the period immediately before the auditor would be rotated. Backward induction would lead to an unraveling of the truth-telling equilibrium. Given a continued restriction on contingent audit fees, mandatory rotation could have the effect of decreasing auditor independence. On the other hand, if the ban on contingent audit fees were removed, then mandatory rotation would not necessarily diminish independence, as the audit committee could adjust its fees to motivate truthful reporting.

In determining whether to retain or replace the incumbent, "[t]he outside auditor [should] discuss with the audit committee the auditor's judgments about the quality, not just the acceptability, of the company's accounting principles. The discussion should include...the degree of aggressiveness or conservatism of the company's accounting principles and underlying estimates" (Blue Ribbon Committee 1999). Whether audit committees have lived up to their responsibility to play an active role in the retention/replacement decision is an open question. Still, the opportunity for disciplining in a multiperiod framework exists, and has similar characteristics to the single period, contingent fee model presented in the previous section.

In our model, firing the auditor is not a sign of dissent. The threat of firing must be credible, and therefore auditors are fired with some probability despite the fact that they accept a truthful report. The model suggests that auditors are most likely fired when outcomes (and thus reported earnings) are extreme. Typically, researchers have assumed that firing is more likely when auditors require the firms to report a low earnings number or when auditors issue qualified reports. Our analysis is consistent with this to a point, but the reason for the negative market reaction would be the fact that earnings are low (or lower than reported), not that the auditor was removed. We also have the flip side: auditors will also be more likely to be fired when reported earnings are very high. It remains an empirical question whether there is a nonmonotonic relationship between firings and earnings, and whether we can identify which firms will be penalized for an auditor switch (i.e., those with correctly low earnings) and which should be rewarded (i.e., those with correctly high earnings) for ensuring the independence of their auditors.

For the remainder of this section, we consider a two-state ($N = 2$) world in which the manager is paid a bonus in the high state only ($z = 1$).

Then, the audit committee's optimization program becomes:

$$\min_{y^s_*, q_i} \quad y^s_* + \frac{g}{2}(2 - q_1 - q_2) \tag{9}$$

$$\text{s.t.} \quad K \leq (y^m_2 - y^m_1) - (q_1 - q_2)\beta V \leq Q^a(1) \tag{10}$$

Since $y^m_1 \leq y^m_2$ and $q_1 \geq q_2$, we set $y^m_1 = 0$ and $q_1 = 1$, their cost-minimizing levels. Because of the structure of the constraints, one of three cases holds. Either the left constraint is binding, the right constraint is binding, or neither constraint is binding. If the left constraint is binding, then we can substitute Equation (7) into that constraint

($K = (y_2^m - y_1^m) - (q_1 - q_2)\beta V$), and rewrite y_*^s in terms of q_2. Then, we have the unconstrained objective function:

$$\underbrace{\frac{(y_2^m - K)(2 - \beta(q_2 + 1)) - y_2^m\beta(1 - q_2)}{2\beta(1 - q_2)}}_{y_*^s} + \frac{g}{2}(1 - q_2). \tag{11}$$

Differentiating and setting equal to zero, we solve for the optimal q_2, where:

$$q_2 = 1 - \sqrt{2\frac{(y_2^m - K)(1 - \beta)}{\beta g}}. \tag{12}$$

Then, substituting q_2 into the expression for y_*^s we have:

$$y_*^s = \frac{\sqrt{2g(y_2^m - K)(1 - \beta)} - \sqrt{\beta}K}{2\sqrt{\beta}}. \tag{13}$$

Similarly, if the $Q^a(1)$ side of the constraint is binding, then the substitution results in the analogous expressions for q_2 and y_*^s, with $Q^a(1)$ in place of K:

$$q_2 = 1 - \sqrt{2\frac{(y_2^m - Q^a(1))(1 - \beta)}{\beta g}}; \tag{14}$$

$$y_*^s = \frac{\sqrt{2g(y_2^m - Q^a(1))(1 - \beta)} - \sqrt{\beta}Q^a(1)}{2\sqrt{\beta}}. \tag{15}$$

The values of q_2 and y_*^s from Equations (14) and (15) give better (i.e., lower) values of the objective function than those in Equations (12) and (13) by the following logic.

The mathematical program is a minimization problem. The q_2 expression (12) is increasing in K and the objective function (9) is decreasing in q_2; similarly, the y_*^s expression (13) is decreasing in K and the objective function is increasing in y_*^s. Finally, $Q^a(1) > K$ (by the assumptions that $Q^a(-1) = -K$, $Q^a(0) = 0$, and $Q^a(j - i)$ is convex in its argument).

The third possibility, that neither constraint is binding, gives us an unconstrained minimization problem, the solution to which is $y_*^s = 0$ and $q_2 = 1$. In other words, the audit committee does not offer compensation and always retains the auditor for the next period. This case will only support a truthful equilibrium if $D_2 < Q^a(1)$ (i.e., if the cost of getting overstatements is too high).

The payment y_*^s from Equation (15) is increasing in the probability of retention. We have an interior solution in which q_2 is strictly less than 1 as long as and $y_2^m > Q^a(1)$ and $\beta/(1 - \beta) \geq 2(y_2^m - Q^a(1))/g$. If g is very low, then the audit committee will prefer to "discipline" the auditor with the threat of firing (i.e., a low q_2). However, as g increases, the audit committee will trade off additional compensation with the probability of firing the auditor to make the shareholders' interests (i.e., truthful reports rather than optimistic reports). The auditor's discount rate also plays a role in the trade-off between q_2 and y_*^s. If the auditor values future revenues from audit services very highly (i.e., high β), then the audit fees in each period will be lower, but the probability of retaining the auditor will increase. Formally:

Proposition 5: In the multiperiod game without contingent audit fees with $N = 2$ and $z = 1$, if $\beta/(1 - \beta) \geq 2(D_2 - Q^a(1))/g$ and $D_2 \geq Q^a(1)$, a truth-telling equilibrium exists.

The audit committee contracts with the auditor using the following compensation/retention scheme:

$$y^s = \left[\frac{\sqrt{2g(D_2 - Q^a(1))(1 - \beta)} - \sqrt{\beta}Q^a(1)}{2\sqrt{\beta}}, \frac{\sqrt{2g(D_2 - Q^a(1))(1 - \beta)} - \sqrt{\beta}Q^a(1)}{2\sqrt{\beta}}\right]$$

$$q = \left[1, 1 - \sqrt{2\frac{(D_2 - Q^a(1))(1 - \beta)}{\beta g}}\right].$$

Managers report truthfully, offering the pair (1, 0) when true earnings are $i = 1$ and $(2, D_2)$ when true earnings are $i = 2$. Auditors accept the contracts and report.

In contrast to the single-period analysis, the audit committee can provide incentives to induce the auditors to accept truthful reports even without contingent fees in the multiperiod analysis. The intuition for this is simple. Since the perpetuity value of an audit engagement is quite high, the threat of firing disciplines the auditor to provide unqualified opinions only when the report is truthful. By basing the decision to retain or fire the auditor on the outcome of the reports, the audit committee can *de facto* make auditor's compensation contingent. Despite the fact that auditors will follow a truth-telling strategy in equilibrium, some auditor switches take place.

The assumption of $N = 2$ allows us to solve for an analytical solution for the audit committee's strategy. For $N > 2$, a truth-telling equilibrium will exist when the optimization problem (8) has a solution, that is, the objective function is continuous and bounded and the feasible region is a non-empty, closed set. The objective function is bounded by zero, and the weak inequalities make the feasible region a closed set. The conditions for existence of a truth-telling equilibrium for general N will therefore be the constraints themselves (i.e., the conditions for a non-empty feasible region).

The infinite horizon is a necessary assumption. If there are T periods in the model, then the auditor cannot be motivated to tell the truth in period T for the reasons given in the single period analysis. Then, in period $T-1$, the auditor knows that the value of retaining the client is zero, since the shareholders will not pay more in the last period than the reservation level of the auditor. So, in period $T-1$ the auditor cannot be motivated with the threat of firing. The entire equilibrium unravels, and a finite period model simplifies to a repeated single period game.

The clean surplus nature of accounting requires accounting numbers to reverse themselves in subsequent periods, reducing the severity of the independence problem. We do not model this additional cost of misreporting in our multiperiod analysis. However, since we focus on truthful equilibria, there is no misreporting and thus no "settling up."

Contingent Audit Fees Allowed

In this section, we allow the audit committee to write contingent contracts with auditors. The cost of such a contract will be (weakly) lower since the audit committee could offer the noncontingent contract given by Proposition 5 to motivate acceptance of truthful reports only. In equilibrium, $y_2^s = 0$ and $q_1 = 1$. Because the manager is attempting to influence the auditor by offering nonaudit revenues for accepting a report of $j = 2$, the shareholders will incur additional costs if they (1) make acceptance of $j = 2$ more attractive (i.e., $y_2^s > 0$) or (2) make acceptance of $j = 1$ less attractive (i.e., $q_1 < 1$). We have the following proposition.

> **Proposition 6:** In the multiperiod game with contingent audit fees allowed with $N = 2$ and $z = 1$, if $D_2 \geq Q^a(1)/2$, a truth-telling equilibrium exists. The audit committee has lower expected costs than under a regime banning contingent fees.

The audit committee contracts with the auditor using the following compensation/retention scheme:

$$y^s = \left[\frac{\sqrt{g(2D_2 - Q^a(1))(1 - \beta)} - \sqrt{\beta}Q^a(1)}{2\sqrt{\beta}}, 0\right];$$

$$q = \left[1, \frac{1}{\beta} - \sqrt{\frac{(2D_2 - Q^a(1))(1 - \beta)}{\beta g}}\right].$$

As before, managers report truthfully, offering the pair (1, 0) when true earnings are $i = 1$ and $(2, D_2)$ when true earnings are $i = 2$. Auditors accept the contracts and report.

Overall, the audit committee is better off when it can offer contingent fees in addition to contingent retention. Since the managers use the same strategy in both cases, the audit committee can always (weakly) improve its objective when fewer constraints are imposed. That is, it could simply resort to the y^s and q from the previous proposition, but chooses a new y^s and q whenever the new compensation/retention pair improves the value of its objective function.

The result that the audit committee's expected costs are reduced generalizes to $N > 2$. The audit committee's optimization problem in the contingent fee regime is a less constrained version of the optimization problem given by Equation (8) in which all the y_j^s can vary.

Numerical Example, continued

Continuing with the parameters given in Table 2, we extend the numerical example for the multiperiod analysis. Let $g = 10{,}000$ and $\beta = .98$; V is calculated from the other parameters.

If the manager offers (1, 0) when earnings $i = 1$, (2, 0) when $i = 2$, (3, 200) when $i = 3$, and (4, 400) when $i = 4$, then the audit committee's best responses as part of a truth-telling equilibrium in the two regimes, contingent and noncontingent fees, are:

Contingent Audit Fees Not Allowed	**Contingent Audit Fees Allowed**
Audit Fee: y^s= [135, 135, 135, 135]	Audit Fee: y^s= [0, 100, 0, 0]
Retention: q = [0.99, 1, 0.99, 0.96]	Retention: q = [1, 1, 1, 0.97]

When contingent audit fees are not allowed, the audit committee can only discipline the auditor by firing him (which is costly to both). By setting compensation to an amount above zero, the audit committee can reduce the frequency with which it fires its auditor, and save significantly. If audit fees were zero, then the value to the auditor of retaining the audit in future periods, regardless of true earnings, goes down. Therefore, firing the auditor is not a particularly effective disciplining mechanism, and thus it must be used much more extensively. In particular, if audit fees are fixed at zero, then the audit committee would have to fire the auditor 25 percent of the time when reported earnings were 4. The cost to the audit committee of the optimal firing rule and compensation above is 294.

If contingent audit fees are allowed, then the audit committee would pay the auditor zero unless reported earnings are 2, in which case the audit fee would be 100. The auditor will never be fired unless reported earnings are 4, in which case the auditor is retained with probability 0.97. The cost to the audit committee in this environment is 88, a significant savings over the noncontingent fee case. The intuition behind the solution is fairly simple. Firing is costly, so it should be done as infrequently as possible. To insure that rejection is not preferred to accepting a truthful report, the value to the auditor must be $(N - 1)K$ in state N. In state 2, the manager is not providing any compensation to the auditor, so the audit committee must pay 100 for the auditor to accept the truthful report. Alternatively, consider an offer of 400 for a report of 4. Here, state 4 is "too appealing," in that the auditor would accept a report of 4, regardless of true earnings. To reduce the value of accepting a report of 4, the audit committee fires the auditor with positive probability when a report of 4 is issued.

V. CONCLUSION

This paper uses a stylized model to explore one way in which an audit committee, acting on behalf of shareholders, can discipline the auditor-manager interactions to ensure truth-telling in audited financial reports. This work is related to the ongoing debate regarding an exclusionary ban of nonaudit services to audit clients. Our results show that in a single-period framework, a less regulatory approach, that of lifting the prohibition on contingent audit fees, can create the desired result of truthful reports. The more regulatory approach, that of banning nonaudit services to audit clients, would not accomplish this. Moreover, professional services firms have been described as starfish,[16] with legs representing the businesses: auditing, tax assistance, risk management, IT consulting, etc. When one leg is lost, a new leg grows back in its place. For example, Arthur Andersen "regrew" nonaudit services after it split with Andersen Consulting. Thus, a ban on currently provided nonaudit services (e.g., management advisory services) cannot anticipate growth in *new* types of nonaudit services (e.g., global risk management).

Further, when we extend the model to a multiperiod framework, we demonstrate the existence of a truth-telling equilibrium both with and without contingent audit fees. In a multiperiod setting, the audit committee essentially has the power to pay contingently, even if the payments in any one period are noncontingent. There is no proscription against the audit committee adjusting the retention rate of auditors based on the outcome of the audit, and a lower retention rate can serve as a punishment to the auditor. However, if the costs of auditor switching is high, then firms would prefer to discipline their auditors via compensation rather than retention rate.

We study both the single-period model and the multiperiod model because the latter unravels in a finite horizon. In fact, some firms have a policy of periodically rotating the

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[16] Rick Antle, Testimony at SEC hearings, Washington D.C., July 25, 2000.

CPA firm that conducts their audit (Addams et al. 1996). At first glance, this policy may appear to enhance auditor independence, but our analysis suggests that firms with such a policy that receive nonaudit services from their auditors are more likely to have overstated earnings, i.e., less independent auditors. It is an empirical question whether audit failures and auditor litigation are more likely for firms with rotation policies.

In both parts of our analysis, the single-period model with contingent audit fees and the multiperiod model with or without contingent audit fees, the audit committee should "punish" (via lower payment or lower retention rate) the auditor for a higher earnings report, i.e., one which garners management a higher bonus. By following such a strategy, the audit committee can ensure truthful reporting. Our analysis is based on the auditors as profit-seeking entities, ignoring the ethical considerations surrounding honesty. Auditing professionals have found this suggestion surprising (among other adjectives), as noncontingent fees have always been believed to promote auditor independence. We feel the changing nature of the auditing environment warrants a reexamination of the ban on contingent fees.

In our model, we assume the following classification of the three players: managers prefer optimistic reports, the audit committee representing shareholders prefers the truth, and auditors prefer understated reports. We recognize that auditors may have incentives not to underreport, including maintaining a good relationship with their clients. For qualitatively similar results, we simply need a divergence between the preferences of managers, shareholders, and auditors. For example, we could assume that managers have a preference for great optimism, shareholders have a preference for slight optimism, and auditors have a preference for the truth.

In this paper, the manager's bonus compensation scheme is exogenously determined. Further work is required to derive the optimal bonus scheme to encourage managerial effort and discourage opportunities for managerial influence over audited reports. Additionally, we have made the implicit assumption that the auditor executes the audit competently, ignoring the auditor's effort decision. Although this is probably a reasonable assumption among a class of audit firms (e.g., Big 4), one could consider motivating auditor inputs rather than focusing solely on outputs. Finally, we have ignored the effects of competition for the audit. An examination of competition could focus on bidding strategies or auditor specialization.

The SEC's debate over auditor independence has polarized the business community. Academic research examining issues of independence has the opportunity to add to the current debate and consider novel remedies. We view this paper as a step toward addressing some of the issues that arise given the current regulatory environment and consider how changes to this environment affect auditor independence and truth-telling.

APPENDIX

Proof of Proposition 1

From (1), the auditor accepts the pair if $y_j^s - y_1^s + y_j^m - y_1^m \geq Q^a(j - i) - Q^a(1 - i)$. The audit committee's payments are normalized to 0 at this point, eliminating the y_j^s and y_1^s terms. If the auditor rejects the proposal, then he refuses both the nonaudit service contract and the report, or $y_1^m = 0$. The auditor's acceptance decision is then: accept if $y \geq Q^a(N - i) - Q^a(1 - i)$. For this to be true for all i, the RHS must be the largest value possible, which occurs at $i = 1$. Therefore, the relevant condition is $y \geq Q^a(N - 1)$.

Proof of Proposition 2

The manager maximizes $(b_j - \alpha y_j^m)A_i^*$ by making a (j_i, y_{ji}^m) offer to the auditor. Compare $(N, Q^a(N - 1))$ and any other acceptable offer $(j', Q^a(j' - 1))$. Note that if the excess nonaudit services offered with a report of j' were less than $Q^a(j' - 1)$ the pair would be rejected. If the manager offers $(N, Q^a(N - 1))$, then his net benefit is $b_N - \alpha Q^a(N - 1)$, whereas if he offers $(j', Q^a(j' - 1))$, his net benefit is $b_{j'} - \alpha Q^a(j' - 1)$. Since $b_N - b_j > \alpha[Q^a(N - 1) - Q^a(j - 1)]$ for all j, it holds for j' and therefore the manager prefers to offer $(N, Q^a(N - 1))$.

Proof of Proposition 3

We present the proof for $D_N \geq K$. The proof for $D_N < K$ is analogous.

Auditor

Acceptance and rejection are the same for $i = 1$. Given the true state is $2 \leq i \leq N - z$, the manager offers $(i,0)$ and the audit committee offers D_N. The value to the auditor of rejecting is the payment in state 1, $D_N - K$ plus the benefit of conservatism, or K, for a net of D_N. The value of accepting is the payment in state i, D_N, plus zero benefit (and zero cost) since the report is truthful. So, the auditor will accept. For states $N - z + 1$ through N, rejection nets the auditor D_N and acceptance nets the auditor $D_N - D_j + D_j = D_N$, and thus the auditor will accept.

Manager

Consider changing the report/nonaudit service pair. If the true state is i, and the manager offers (j, D_j), then the auditor will accept. This provides no net benefit to the manager (he receives b_j, but it is consumed by the necessary payments to the auditor), and, thus, we can rule out this defection. If the manager offers $y < D_j$ with a report of j, then the auditor will reject. If the manager maintains a truthful report, but tries to shave the cost of the nonaudit services, then the pair will be rejected.

Audit Committee

For $D_N > K$, the audit committee makes positive payments in states $1,\ldots,N - 1$. The payments are increasing from states 1 through $N - z$, and then decreasing from states $N - z$ to N. Consider a reduction in any single payment.

$j = 1 \Rightarrow$ Auditor accepts overreport	Minimum cost $Q^s(1) > K$
$j \in \{2,\ldots,N - z\} \Rightarrow$ Auditor rejects	Savings K
	Minimum cost $Q^s(-1) > K$
$j \in \{N - z,\ldots,N\} \Rightarrow$ Auditor rejects	Savings $< K$
	Minimum cost $Q^s(N - z) > K$

It follows, using the same reasoning as above (and the fact that the payment in state N is already zero) that no reductions in a combination of states is preferred. Since the equilibrium has truthful reporting, the audit committee does not want to increase any payment.

Proof of Proposition 4

Because the manager receives no bonus in state $j \in \{1,\ldots,N - z\}$, reports in this region will come with zero nonaudit service levels ($y_j^m = 0$). The first set of constraints can be reduced to:

$$q_j\beta V \geq q_1\beta V + K \qquad \text{for} \quad j \in \{2,\ldots,N - z\} \tag{16}$$

It follows directly that $q_2 > q_1$. The second set of constraints can be reduced to:

$$q_j\beta V \leq q_1\beta V + K + Q^a(1) \qquad \text{for} \quad j \in \{2,\ldots,N - z\} \tag{17}$$

Because of the cost (g) associated with an auditor switch, the cost minimizing q_j will be at the top of the range allowed by the constraint. That is the constraint will be binding, so $q_j\beta V = q_1\beta V + K + Q^a(1)$ for non-bonus states. The equation does not depend on j in this range, so the q_j s are flat.

For bonus states, the second set of constraints includes a non-zero y_j^m:

$$q_j\beta V \leq q_1\beta V + K + Q^a(1) - y_j^m \qquad \text{for} \quad j \in \{N - z + 1,\ldots,N\} \tag{18}$$

The constraint will once again be binding, and because the manager's bonus is increasing in j, the y_j^m are increasing in j, so the q_j are decreasing in j for states $N - z + 1$ through N.

Proof of Proposition 5

The audit committee's payment and retention schemes obtain by substituting D_2 for y_2^m in Equations (12) and (13) and is therefore by definition the audit committee's optimal response to the manager's offering (1, 0) when $i = 1$ and $(2, D_2)$ when $i = 2$. Equation (10) is generated by combining the two inequality constraints that the auditor (a) accepts a report of $j = 1$ when $i = 1$ and (b) accepts a report of $j = 2$ when $i = 2$. Given the constraints are met, the auditor will accept the truthful reports. The manager does not want to deviate from this strategy: if he offers (2, y) with $y < D_2$, then the auditor will reject. If $y > D_2$, then he incurs additional (unnecessary) compensation costs. If he offers (1, y) where $y > 0$, then the auditor will accept, but the manager gets no bonus and incurs a compensation cost. He cannot reduce y below zero.

Proof of Proposition 6

This result is derived in a similar manner to Proposition 5. We start with the optimization problem:

$$\min_{y_1^s, q_2} \ \frac{y_1^s}{2} + \frac{g}{2}(1 - q_2) \tag{19}$$

$$\text{s.t.} \quad K \leq (1 - q_2)\beta V \leq (y_2^m - y_1^s) - Q^a(1) \tag{20}$$

(similar to Equations (9) and (10)). As before, the right-hand constraint will be the one that binds. Solving for V from Equation (5), we get:

$$V = \frac{y_1^s + y_2^m}{2 - \beta(1 + q_2)} \tag{21}$$

Substituting Equation (21) into the constraint above and solving for y_1^s, we get:

$$y_1^s = \frac{2y_2^m(1 - \beta) - 2Q^a(1) + \beta Q^a(1) + \beta q_2 Q^a(1)}{2(1 - \beta q_2)} \tag{22}$$

Now the objective function can be written as a function of q_2 only. The value-maximizing q_2 is given in the proposition. The y_1^s comes from substituting the optimal value of q_2 into Equation (22), with $y_2^m = D_2$.

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The Effects of Industry Specialization on Audit Risk Assessments and Audit-Planning Decisions

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ABSTRACT: This study investigates the effects of industry specialization on auditors' risk assessments and audit-planning decisions. In an experiment, auditors from different industry specializations complete a hypothetical audit case set in a specific (bank) industry, which creates either a match or a mismatch between the auditors' industry specialization and the hypothetical client's industry. Furthermore, I manipulate the industry-specific case information to achieve differential audit risk levels. I also provide the auditors with a set of preliminary audit procedures and a constrained time budget. I find that the auditors' knowledge of the client's industry improves their audit risk assessments and directly influences the nature and the perceived quality of their audit-planning decisions. In addition, the auditors' knowledge of the client's industry moderates the sensitivity of the auditors' planning decisions to their audit risk assessments.

Keywords: *industry specialization; audit risk assessment; audit-planning decisions; auditors' performance.*

Data Availability: *Available upon request.*

I. INTRODUCTION

Recognizing the increasing complexities of today's business in a knowledge-based economy, many public accounting firms have reorganized their staff and practice by industry (Bell et al. 1997; Emerson 1993). Consequently, today's auditors' training and experience are becoming more industry-specific earlier in their audit career. While early industry specialization could potentially reduce auditors' breadth of general audit experience, industry-specialist auditors have greater opportunities to develop more in-depth knowledge of the industry in which they specialize. Such in-depth knowledge should lead to greater audit effectiveness.

Prior research on auditors' industry experience and/or specialization has found that auditors with greater industry-specific experience generally possess greater knowledge of

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the industry related to their experience. However, few studies have examined whether and how industry knowledge acquired through industry experience translates into more effective audit performance.[1] In this study, I contribute to the extant literature by investigating the effects of industry specialization on auditors' risk assessments and audit-planning decisions as well as the relationship between auditors' risk assessments and their planning decisions.

Examining whether auditors' industry specialization leads to better audit risk assessment is important because auditors' risk assessments will influence the subsequent design and conduct of an audit. During the early stages of an audit, inappropriate audit risk assessments can result in misdirected audit resource allocation and, ultimately, in an ineffective and/or inefficient audit. I posit that auditors' knowledge of the client's industry positively affects auditors' ability to assess audit risks.[2] In addition, I posit that the manner and extent to which auditors change planned audit procedures and the quality of auditors' planning decisions are affected by their knowledge of the client's industry and/or risk assessment.

In an experiment, auditors from different industry specializations complete a hypothetical audit case set in the banking industry, which creates either a match or mismatch between the auditor's industry specialization and the hypothetical client's industry. Further, I manipulate the industry-specific case information between the auditors to achieve differential audit risk levels. I also provide the auditors with a set of preliminary audit procedures and a constrained time budget.[3] I find that the auditors' knowledge of the client's industry improves their audit risk assessments. The auditors' knowledge of the client's industry also directly affects the nature and the perceived quality of changes made by the auditors to the planned audit procedures and time budgets. Auditors working in their industry specialization modify the nature of the planned audit procedures more than those working outside their industry specialization. Industry experts judge the quality of audit procedure changes as well as the final audit programs and time budgets proposed by industry-specialist auditors to be higher than those of auditors working outside their industry specialization. Last, the extent of audit procedure changes made by industry-specialist auditors is sensitive to their audit risk assessments, but not for auditors who are working outside their industry specialization. Overall, the results have important implications for audit effectiveness and efficiency as auditors' knowledge of the client's industry is found to affect not only the auditors' risk assessments, but also the nature, quality, and risk-sensitivity of their planning decisions.

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[1] One notable exception is Owhoso et al. (2002). The study's findings reveal that audit seniors and managers, working on an individual basis or in a team consisting of a senior and a manager, detect more mechanical and conceptual errors when reviewing working papers of a client in (rather than outside) the industry of the auditors' specialization.

[2] I use the term "knowledge of the client's industry" to refer to auditors' knowledge of accounting, auditing, and business environment that is specific to a client's industry (as opposed to knowledge that is applicable across industries). This definition is consistent with prior research that distinguishes three categories of auditors' knowledge—accounting, auditing, and business-environment knowledge (Bonner and Lewis 1990; Solomon et al. 1999). Further, each category of knowledge may be general or industry-specific (Waller and Felix 1984a, 1984b; Wright and Wright 1997).

[3] In practice, every audit engagement has a time budget, which imposes time constraints on the auditors. Consequently, when designing and conducting an audit, auditors are concerned not only with audit effectiveness but also with audit efficiency. Presenting the study participants with a constrained time budget reinforces the participants' concern for audit efficiency. It also enables the study to contribute to extant time-constraint literature by examining the effects of auditors' industry specialization and risk assessment on auditors' planning response to an impending time constraint. Extant time-constraint studies in auditing (see review by DeZoort and Lord 1997) have largely focused on auditors' behaviors and performance under time constraints as opposed to auditors' response to impending time constraints (Solomon and Brown 1992).

The remainder of the paper is organized as follows. Section II reviews the prior literature and develops the hypotheses. In Section III, I describe the experiment. Section IV reports the results. In Section V, I present concluding comments.

II. HYPOTHESIS DEVELOPMENT

Prior research on auditors' industry-specific experience suggests that auditors with greater industry-specific experience possess more complete knowledge as well as more accurate frequency knowledge of plausible business environment explanations for unexpected ratio fluctuations (Solomon et al. 1999), and generate more industry-unique financial statement error hypotheses (Wright and Wright 1997). Research shows that auditors with greater industry-specific experience more frequently recognize the correct error-cause of unexpected financial statement balance fluctuations (Bedard and Biggs 1991) and are more likely to correctly detect management fraud (Johnson et al. 1991). Bonner and Lewis (1990) find industry-specific experience to be a significant predictor of auditors' performance in tasks more commonly encountered in a specific industry. However, Ashton (1991) finds no strong positive relationship between auditors' industry-specific experience and the accuracy of their error frequency knowledge.

Overall, the studies on auditors' industry-specific experience suggest that auditors develop more extensive knowledge of the industry in which they specialize or have greater audit experience. Consistent with these findings, Bell et al. (1997) suggest that auditors develop extensive knowledge of their clients' businesses and industries, and that this knowledge contributes significantly to the auditors' ability to detect anomalies and verify consistencies. This reasoning implies that industry-specialist auditors are likely to better recognize the audit risks associated with an engagement when they face a client in that industry as opposed to another industry. For example, auditors' knowledge of a client's industry enables them to benchmark the client's performance against its industry. The pattern and/or levels of the client's performance measures against industry norms can be diagnostic of the audit risks associated with the client.

I am aware of only two studies that have investigated the effects of industry-specific experience on auditors' audit risk assessments (Wright and Wright 1997; Taylor 2000). The findings of both studies on improved risk assessments based on industry-specific experience, however, are not conclusive. Wright and Wright (1997) find that, contrary to their expectations, auditors' industry-specific experience is not significantly associated with their audit risk assessments and budgeted hours for the error-embedded, industry-specific accounts. Taylor (2000) finds that auditors with less bank audit experience assess inherent risk generally higher than auditors with greater bank audit experience, and that the difference is greater for bank-specific accounts than for generic accounts. However, Taylor (2000) does not examine or discuss whether the industry specialists' lower risk assessments are driven by or consistent with the risk factors in the audit case.

To test the proposition that auditors' knowledge of the client's industry improves auditors' ability to discern audit risks (formally presented below), I employ two hypothetical audit cases with embedded industry-specific information that signal different audit risk levels (higher versus lower). As part of the audit-risk-level manipulation, I also provide auditor-participants with a subordinate's risk assessment that is, unknown to the participants, incorrect (i.e., the subordinate's risk assessment is lower in the higher-risk case than the lower-risk case). Prior studies in psychology and auditing find that when initial anchors are available, individuals make their judgments by adjusting from the initial anchors but these adjustments are generally inadequate (Slovic and Lichtenstein 1971; Tversky and Kahneman 1974; Smith and Kida 1991). Thus, the subordinate's risk assessments are likely to influence the participants' risk assessments, but the strength of the anchor's influence is likely to depend on whether the participants realize that the subordinate's risk assessments are incorrect. Industry-matched auditors, possessing greater knowledge of the client's industry, are likely to recognize the industry-specific information indicative of higher or lower audit risk and realize that the subordinate's risk assessments are incorrect.[4] On the other hand, industry-mismatched auditors who lack knowledge of the client's industry are expected to anchor on the subordinate's incorrect risk assessments and fail to adequately adjust their risk assessments to the industry-specific information indicative of higher or lower audit risk.

> **H1:** Industry-matched auditors better discern audit risks of a higher-risk engagement and a lower-risk engagement than do industry-mismatched auditors.

In practice, when designing the audit programs for an engagement, auditors either develop the audit programs on their own or rely on standard audit programs adopted by their firms and/or prior years' audit programs (i.e., inherited audit programs). In this study, I examine the changes auditors make to an inherited audit program and time budget. I provide the participants with a set of planned audit procedures, including the assigned auditor-rank level and budgeted hours for each audit procedure, which is described as developed by a subordinate from the prior year's audit.[5]

When customizing inherited audit programs and time budgets to a client's current circumstances, auditors generally can make three kinds of changes: procedure-changes, staff-changes, and/or hour-changes. A procedure-change involves modifications to the planned audit procedures, which can take the form of adding new procedures, replacing planned procedures, deleting planned procedures, and changing sample sizes of planned procedures. A staff-change involves changes to the assigned staff level without any change in the planned procedures. The assigned staff level may be substituted with more or less senior staff level. Finally, an hour-change involves changes to the budgeted audit hours without any change in the planned procedures and assigned staff level.

Of these three changes, a procedure-change is more heavily reliant on auditors' (auditing) knowledge of the client's industry than either a staff-change or an hour-change. This greater knowledge dependency is because in addition to potential changes to the assigned staff level and budgeted hours, the auditor must specify the nature, timing, and/or extent of planned audit procedures. To be able to do this, an auditor must have an appreciation of the audit tests that should or could be performed for a client in a specific industry. Consequently, I posit that industry-matched auditors, possessing more extensive (auditing) knowledge of the client's industry, have greater ability than do industry-mismatched auditors to make procedure-changes when customizing inherited audit programs.

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[4] I use the term "industry-matched auditors" to refer to auditors whose industry specialization matches the client's industry, and the term "industry-mismatched auditors" to refer to auditors whose industry specialization does not match the client's industry.

[5] By providing participants with a common audit program and time budget (as opposed to participants developing audit program and time budget on their own), and observing the changes that participants make to the provided audit program and time budget, I am better able to track and identify the differences in participants' audit-planning decisions associated with their industry specialization. However, modifying an inherited audit program and time budget is probably less difficult than developing an audit program and time budget from scratch. Thus, my approach is likely to weaken and bias against my ability to detect audit-planning differences between auditors of different industry specializations.

H2: Industry-matched auditors make more procedure-changes than do industry-mismatched auditors.

When customizing inherited audit programs and time budgets to a client's current circumstances, auditors are generally concerned not only with audit effectiveness, but also with audit efficiency. Auditors will want to minimize any perceived inefficiency in the planned audit procedures and time budgets.[6] However, perceived inefficiencies in the planned audit procedures and time budgets are likely to be inversely related to the auditors' risk assessments because as audit risk increases, more audit work generally is required. Thus, *ceteris paribus*, the higher the assessed audit risks, the lower the assessed inefficiencies in the planned audit procedures and time budgets.

Audit procedures, however, vary in their effectiveness and importance in addressing audit risk. For auditors to make procedure-changes, staff-changes, and hour-changes that are sensitive to their audit risk assessments (i.e., more audit resources are assigned as perceived audit risk increases), they need to evaluate the audit risks as well as the relevant audit procedures to address the risks. Both steps require auditors to possess extensive (business-environment and auditing) knowledge of the client's industry. Consequently, I posit that industry-matched auditors' procedure-changes, staff-changes, and hour-changes are more sensitive to their risk assessments than are those of industry-mismatched auditors.

H3: Industry-matched auditors' procedure-changes, staff-changes, and/or hour-changes are more sensitive to their audit risk assessments than are those of industry-mismatched auditors.

Industry-matched auditors' more extensive knowledge of the client's industry not only enables them to better assess audit risks, but also to better decide when and where it is suitable to make procedure-changes, staff-changes, and/or hour-changes. For instance, industry-matched auditors should better understand how to modify planned audit procedures, what level of audit staff possesses the required skills and knowledge, as well as the amount of time required for performing the planned audit procedures. In addition, industry-matched auditors should better understand the mix or combination of audit tests and audit resources (staff level and hours) to address the risks associated with an audit engagement. As a result, the quality of industry-matched auditors' final audit programs and time budgets also is likely to be better than that of industry-mismatched auditors. For the purposes of this study, industry experts from the participating firm will judge the quality of the participants' audit procedure changes, final audit programs and time budgets based on the given audit engagement's underlying circumstances and risks.[7]

H4: The perceived quality of industry-matched auditors' procedure-changes, staff-changes, hour-changes, final audit programs, and/or final time budgets is higher than that of industry-mismatched auditors.

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[6] Efficiency refers to the achievement of desired objectives with a minimum expenditure of resources. Inefficiency, therefore, is an excess employment of resources (i.e., beyond the minimum needed) to achieve desired objectives. In the context of my study, inefficiency refers to the extent of redundancy in the planned audit procedures, the assigned staff level, and the budgeted hours to conduct a proper audit.

[7] The focus in H4 is on the perceived quality of auditors' audit procedures, final audit programs, and final time budgets in addressing the audit risks underlying an audit engagement. On the other hand, H3 focuses on the sensitivity of auditors' planning decisions to their risk assessments per se (rather than the underlying audit risks of an audit engagement).

III. RESEARCH METHOD

Participants

Participants are 98 supervising audit seniors from a then Big 5 firm recruited at two in-house training sessions. I use the participating audit firm's industry designation of its auditors as the basis for determining participants' industry specialization. Given that the audit case used in the study is set in the banking industry, there are 36 industry-matched auditors who are firm-designated bank specialist auditors. The remaining 62 participants are classified as industry-mismatched auditors. They are firm-designated specialist auditors in industries other than the banking industry (such as real estate, healthcare, consumer and industrial markets, information, communication and entertainment, and manufacturing, retail, and distribution).

The industry-matched participants do not differ significantly from the industry-mismatched participants in the terms of total audit experience (37.81 versus 40.00 months, $p = 0.312$), industry specialization designation (27.25 versus 31.39 months, $p = 0.119$), and audit experience in their industries of specialization (30.86 versus 32.58 months, $p = 0.487$). The industry-matched participants spent a significantly greater percentage of their audit time in the preceding three years on banking clients (such as commercial banks, mortgage banks, credit unions, thrifts, and savings and loans institutions) than did the industry-mismatched participants (73.39 versus 2.87 percent, $p = 0.000$). The above results are similar for both versions of the audit case (i.e., the higher-risk case and the lower-risk case).

Materials

The experimental materials include an audit case that contains background information on a bank-client and some financial information relating to the client's loan portfolio.[8] The case also contains a set of planned audit procedures and the related time budget for the client's loan portfolio.[9] The time budget reveals that the total budgeted cost exceeded the total projected audit fee. The total projected audit fee is set at 65 percent of the total budgeted cost. From discussions with senior bank audit managers, 65 percent is deemed to be sufficiently low to cause participating auditors to be concerned with the engagement's efficiency, but not so low that the engagement is perceived to be an impossible undertaking. The participating firm's realization percentage for audits (including bank audits) ranges from 50 percent to 90 percent. In addition, the audit case states that the audit engagement partner is concerned with the potential budget-overrun and that the partner believes that the engagement efficiency can be improved. The time budget deficit and the partner's concern are meant to reinforce participants' concern for audit efficiency in addition to effectiveness when finalizing the audit program and time budget.

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[8] The general instructions of the experimental materials included an accountability requirement to ensure that the participants took the experimental tasks seriously. Participants were asked to print their name and business contact and were instructed that a professional from their firm would review their responses. They were told that a random sample of their responses would be selected. If they were selected, then they would be contacted concerning their responses. After the experiment, ten participants (10 percent) were randomly selected and provided with the summarized responses of their own and their treatment group.

[9] The assigned staff level and budgeted hours were also specified for each audit procedure, which added up to the total budgeted hours for each staff level in the time budget provided in the case. The budgeted hours are converted into budgeted costs at the hourly billing rate of the respective staff levels. The budgeted hours for the preliminary audit procedures are based on two senior bank audit managers' estimates for a bank of a size similar to the hypothetical bank in the case. The budgeted hour estimates were inflated by an average of 10 percent to allow for some slack.

There are two versions of the audit case. The two versions are identical except for some industry-specific information on the loan portfolio, which I manipulate between participants to achieve differential audit risk levels (higher versus lower). The industry-specific information as well as the audit procedures and the time budget presented in the case were developed in consultation with two senior bank audit managers. A summary of the industry-specific information used to achieve the differential audit risk levels is presented in the Appendix.

As part of the risk manipulation, the case information includes a subordinate's risk assessment that is, unknown to the participants, in a direction opposite to the manipulated risk level of the case (i.e., incorrect). The subordinate's risk assessment is lower in the higher-risk case than in the lower-risk case.[10] The subordinate's risk assessment is stated as 3.5 on a scale from 0 (very low risk) to 10 (very high risk) in the higher-risk case and 7.5 in the lower-risk case. I expect industry-matched auditors to rely more on their knowledge of the client's industry than on the subordinate's risk assessment when assessing audit risk. Industry-matched auditors' knowledge of the client's industry would enable them to recognize whether the industry-specific information indicates a higher or lower audit risk level. On the other hand, industry-mismatched auditors are more likely to be influenced by the subordinate's risk assessment as they lack the relevant knowledge of the client's industry to assess audit risk. As a result, industry-matched (industry-mismatched) auditors' risk assessments are likely to be higher (lower) for the higher-risk case than the lower-risk case.

Procedure

I randomly assigned the participants to complete either the higher-risk case or the lower-risk case. I first presented the participants with background and financial information in the audit case and asked them to assess the audit risk that the client's current net loan balance was significantly misstated on an 11-point scale. After that, I presented the participants with a list of the audit procedures and the related time budget for the client's loan portfolio, and asked them to finalize the audit procedures and the time budget.

An independent coder (with five years of audit experience) and I coded the participants' changes to the planned audit procedures into procedure-changes, staff-changes, and hour-changes. The independent coder had no knowledge of the hypotheses. Both coders were blind to the participants' treatment condition and identity during the coding. Kappa (inter-rater agreement) coefficient for the coding of the participants' changes into procedure-changes, staff-changes, and hour-changes was 0.94 ($p = 0.000$). Coding disagreements were resolved through discussion. I also compiled the final time budget for each participant based on the participant's final audit program, which included the planned audit procedures that were not modified as well as those that the participant modified.

Two senior bank audit managers and two bank audit partners who were not involved in the audit case development independently assessed the quality of each participant's audit procedure changes, final audit programs, and final time budgets using an 11-point scale. The four assessors had average bank audit experience of 148.5 months. The assessors were not aware of the participants' actual identities and industry specialization (i.e., whether the participants were industry-matched or industry-mismatched). The transcribed participants' responses were randomly ordered for the assessment.

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[10] The presence of initial audit anchors (such as the subordinate's risk assessment, preliminary audit program, and budget depicted in the study) is not uncommon in practice. The initial anchors may come from members of the engagement team or prior year working papers. In the study, the initial anchors serve two specific purposes. One, they provide all of the participants within each risk condition with a common starting point, which allows me to better detect relative performance differences between industry-matched and industry-mismatched auditors. Two, initial audit anchors may or may not be correct. When initial anchors are available, incorrect anchors pose greater threats to the audit than correct anchors as prior literature finds that individuals often make inadequate adjustments from initial anchors. Thus, the positive effect of industry specialization, if any, is more beneficial and critical in situations where initial anchors are incorrect than correct. The incorrect initial anchors in the study, therefore, create an interesting and important context for investigating industry specialization effects.

The assessors were instructed to exercise their review skills and knowledge just as they would normally do in practice. I asked the assessors to identify the considerations that they took into account in their quality ratings after they had completed some trial assessments. The identified considerations generally suggested that the assessors were concerned with the impact of the participants' audit procedure changes, final audit program, and final time budget on the audit engagement's effectiveness and efficiency.

For the quality ratings, I organized the four assessors into two groups. Each group consisted of a senior bank audit manager and a senior bank audit partner. One group of assessors judged the participants' transcribed responses for the higher-risk case and the other group judged the participants' transcribed responses for the lower-risk case. While the review arrangement confounded audit case risk level with assessor group, it was selected to minimize the review task complexity and the individual time commitments of the assessors, who were very senior industry auditors from the participating firm. To address the impact of confounding audit case risk level with assessor group on the assessors' quality ratings, I performed two additional analyses. One analysis involved standardizing the raters' quality ratings and the other involved conducting independent-samples t-tests within each audit case risk level. The results are reported in Section IV.

IV. ANALYSIS AND RESULTS

Participants' Audit Risk Assessments

In H1, I predict that industry-matched auditors better discern audit risks of a higher-risk engagement and a lower-risk engagement than do industry-mismatched auditors. The results of an ANOVA with the participants' audit risk assessments as dependent variable and industry specialization (matched versus mismatched) and audit case (higher-risk versus lower-risk) as independent variables reveal a significant industry specialization by audit case interaction ($F_{1,97} = 18.67$, $p = 0.000$, Panel B of Table 1).

The industry-matched (industry-mismatched) participants' mean audit risk assessments for the higher-risk case and the lower-risk case are 5.55 (4.41) and 4.37 (5.60), respectively (Panel A, Table 1). The industry-matched (industry-mismatched) participants' average audit risk assessment for the higher-risk case is higher (lower) than for the lower-risk case (Panel C, Table 1). Thus, the results support H1 and suggest that industry-matched auditors are better than industry-mismatched auditors at discerning differential audit risk levels using industry-specific information.

Participants' Changes to Planned Audit Procedures

The participants' changes to the planned audit procedures are classified into procedure-changes, staff-changes, and hour-changes. I measure the extent of each of these changes in terms of the change in initial budgeted cost.[11] Table 2 shows the change in budgeted cost associated with the three types of changes made by the participants to the planned audit procedures. Note that, on average, the participants' changes to the planned audit procedures

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[11] In the paper, I report only the budgeted cost changes to avoid cluttering. Results of the analyses reported in the paper are qualitatively the same using budgeted staff hour changes as the dependent measure, but some of the results are statistically weaker using the number of times that changes are made.

TABLE 1
Effects of Auditors' Industry Specialization on Audit Risk Assessments

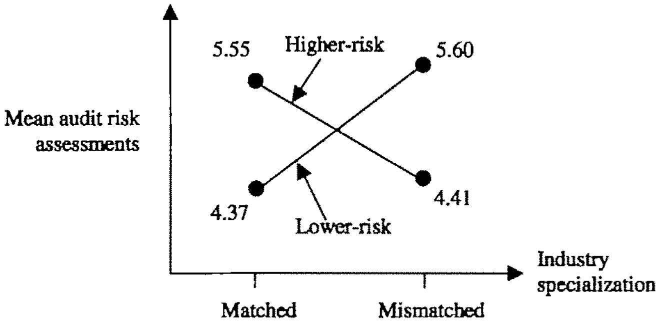
Panel A: The Participants' Mean Audit Risk Assessments (standard deviations in parentheses)[a]

	Industry-Matched Participants	Industry-Mismatched Participants	Combined
Higher-risk case	5.55 (1.19) n = 19	4.41 (1.22) n = 31	4.84 (1.32) n = 50
Lower-risk case	4.37 (0.92) n = 17	5.60 (1.60) n = 31	5.17 (1.51) n = 48
Combined	4.99 (1.21) n = 36	5.01 (1.53) n = 62	5.00 (1.42) n = 98

Panel B: ANOVA of the Participants' Audit Risk Assessments

Source	Degree of Freedom	F-Value	p
Industry specialization	1	0.032	0.858
Audit case	1	0.001	0.980
Industry specialization * Audit case	1	18.673	0.000

Panel C: Graph of the Participants' Mean Audit Risk Assessments



[a] Participants assessed audit risk on an 11-point scale with endpoints labeled 0 = very low and 10 = very high. Participants were told that a subordinate assessed audit risk as 3.5 on the 11-point scale in the higher-risk case and 7.5 in the lower-risk case.

result in reductions of the initial budgeted cost. The participants' budgeted cost reductions suggest that the participants responded to the constrained time budget and the engagement partner's concern with improving the engagement efficiency as stated in the case.

In H2, I posit that industry-matched auditors make more procedure-changes than do industry-mismatched auditors. The results of the ANCOVA with the participants' cost reduction via procedure-changes as dependent variable, the participants' audit risk assessments as covariate, and industry specialization and audit case as independent variables reveal only a significant main effect for industry specialization ($F_{1,97} = 4.015$, $p = 0.048$, Panel B of Table 2). On average, the industry-matched (industry-mismatched) participants' procedure-changes reduce the budgeted cost by $2,444 ($1,750) (Panel A, Table 2). Thus, the results support H2.[12]

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[12] The participants' procedure-changes can be further classified into replacement, deletion, and change of sample sizes of the planned procedures. On average, the industry-matched participants replace and delete planned procedures more but change sample sizes of planned procedures less than the industry-mismatched participants ($696 versus $420 for replacement of planned procedures, $920 versus $496 for deletion of planned procedures,

(*continued on page 211*)

TABLE 2
Effects of Auditors' Industry Specialization on Audit Procedure Changes

Panel A: The Participants' Changes to the Planned Audit Procedures (standard deviations in parentheses)

	Industry-Matched Participants			Industry-Mismatched Participants		
	Higher-Risk	Lower-Risk	Combined	Higher-Risk	Lower-Risk	Combined
	(n = 19)	(n = 17)	(n = 36)	(n = 31)	(n = 31)	(n = 62)
Mean Budgeted Cost Change ($)						
Procedure-changes[a]	−2272 (1824)	−2635 (2140)	−2444 (1959)	−1794 (1675)	−1706 (1194)	−1750 (1444)
Staff-changes[b]	−239 (1113)	−915 (928)	−558 (1071)	−658 (1419)	−427 (1741)	−542 (1579)
Hour-changes[c]	−1863 (2079)	−2780 (2142)	−2297 (2129)	−2300 (2428)	−2350 (2159)	−2325 (2278)
Total changes	−4374 (2737)	−6330 (2764)	−5299 (2886)	−4752 (3150)	−4483 (3428)	−4617 (3268)

Panel B: ANCOVA of the Participants' Procedure-Changes

Source	Degree of Freedom	F-Value (p)
Audit risk assessment (covariate)	1	2.604 (0.110)
Industry specialization	1	4.015 (0.048)
Audit case	1	0.160 (0.690)
Industry specialization * Audit case	1	0.004 (0.952)

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[a] Procedure-changes refer to modifications made by the participants to the planned audit procedures such as addition of new procedures, replacement of planned procedures, deletion of planned procedures, and change of sample sizes of planned procedures.

[b] Staff-changes refer to changes made by the participants to the assigned staff level without any change in the planned procedures. The assigned staff level may be substituted with either more or less senior staff level.

[c] Hour-changes refer to changes made by the participants to the budgeted audit hours without any change in the planned procedures and assigned staff level.

Additional ANOVA with the participants' cost reduction via staff-changes or hour-changes as dependent variable and industry specialization and audit case as independent variables reveal no significant main or interaction effect.[13] On the other hand, ANOVA with the participants' total cost reduction via procedure-changes, staff-changes, and hour-changes as dependent variable and industry specialization and audit case as independent variables reveal an interaction effect significant at $p = 0.091$ ($F_{1,\ 97} = 2.915$). Follow-up analyses reveal that the industry-matched participants' total cost reduction is significantly greater for the lower-risk case ($6,330) than the higher-risk case ($4,374) ($t = 2.13$, two-tailed $p = 0.04$). However, there is no significant difference in the industry-mismatched participants' total cost reduction for the higher-risk case ($4,752) and the lower-risk case ($4,483) ($t = 0.374$, two-tailed $p = 0.748$).

Further analyses reveal that the industry-matched participants make significantly more procedure-changes ($2,444) than staff-changes ($558) ($t = 4.930$, $p = 0.000$). The industry-matched participants also make more procedure-changes ($2,444) than hour-changes ($2,297), but the difference is not significant ($t = 0.275$, $p = 0.393$). Like the industry-matched participants, the industry-mismatched participants make significantly more procedure-changes ($1,750) than staff-changes ($542) ($t = 4.722$, $p = 0.000$). However, the industry-mismatched participants make more hour-changes ($2,325) than procedure-changes ($1,750) and the difference is significant at $p = 0.054$ ($t = 1.637$). The latter result is consistent with the explanation that industry-mismatched auditors lack relevant knowledge of the client's industry, which inhibits their ability to make procedure-changes. Overall, the results suggest that auditors' knowledge of the client's industry affects the manner and the extent to which auditors change planned audit procedures.

Sensitivity of the Participants' Audit Procedure Changes to Their Audit Risk Assessments

I predict in H3 that the sensitivity of auditors' audit procedure changes to their audit risk assessments is greater for industry-matched auditors than for industry-mismatched auditors. To test the hypothesis, I first regress all the participants' total cost reductions (i.e., the sum of cost reductions via procedure-changes, staff-changes, and hour-changes) on their industry specialization (a dummy variable), their risk assessments, and the interaction term between the participants' industry specialization and their audit risk assessment. The regression results reveal that the interaction between audit risk assessment and industry specialization is significant at $p = 0.080$.

Next, I regress the industry-matched participants' total cost reductions on their audit risk assessments. The industry-matched participants' total cost reductions are associated with their audit risk assessments (β coefficient $= -978.46$, $p = 0.013$). The association is negative, which implies that the industry-matched participants reduce the total cost by smaller amounts as their audit risk assessments increase. On the other hand, the regression

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Footnote 12, continued

and $828 versus $834 for change of planned procedure sample sizes). The results of a MANOVA with the participants' cost reduction via the three types of procedure-changes as multiple dependent variables, and industry specialization and audit case as independent variables reveal a main effect of industry specialization, significant at $p = 0.098$. The other main and interaction effects are not significant ($p > 0.345$). Only the univariate ANOVA with the participants' cost reduction via deletion of planned procedures as dependent variable, and industry specialization and audit case as independent variables reveal a main effect of industry specialization significant at $p = 0.077$. The other main and interaction effects are not significant ($p > 0.111$).

[13] A MANOVA with the participants' cost reduction via procedure-changes, staff-changes, and hour-changes as multiple dependent variables, and industry specialization and audit case as independent variables reveals no significant main or interaction effect ($p > 0.263$).

of the industry-mismatched participants' total cost reductions on their audit risk assessments reveal no significant association between their total cost reductions and their audit risk assessments (β coefficient = −89.69, p = 0.745). Thus, industry-matched auditors' total cost reductions are sensitive to their audit risk assessments, but industry-mismatched auditors' total cost reductions are not risk-sensitive (consistent with H3).

I repeat the above two-stage regression analyses for each type of audit procedure change (i.e., procedure-changes, staff-changes, and hour-changes) made by the participants. The first-stage regression results reveal a significant interaction between audit risk assessment and industry specialization for procedure-changes and staff-changes ($p \leq 0.08$), but not for hour-changes ($p = 0.976$).

The second-stage regression results reveal that the industry-matched participants' cost reductions via procedure-changes (β coefficient = −506.15, p = 0.063) and staff-changes (β coefficient = −289.84, p = 0.051) are associated with their audit risk assessments. The industry-matched participants make fewer procedure-changes and staff-changes as their audit risk assessments increase. On the other hand, the industry-matched participants' cost reductions via hour-changes are not significantly associated with their audit risk assessments (β coefficient = −182.47, p = 0.547).

The second-stage regression results reveal that the industry-mismatched participants' cost reductions from procedure-changes (β coefficient = −92.18, p = 0.449), staff-changes (β coefficient = 196.04, p = 0.138), and hour-changes (β coefficient = −193.55, p = 0.313) do not vary significantly with their audit risk assessments. The above results indicate that industry-matched auditors' procedure-changes and staff-changes are more sensitive to their audit risk assessments than those of industry-mismatched auditors. However, both industry-matched and industry-mismatched auditors' hour-changes are not sensitive to their audit risk assessments. Thus, H3 is supported only for procedure-changes and staff-changes but not for hour-changes.

Perceived Quality of the Participants' Audit-Planning Decisions

In H4, I hypothesize that the quality of industry-matched auditors' audit procedure changes, final audit programs, and final time budgets, as perceived by industry experts, is higher than that of industry-mismatched auditors. Panel B of Table 3 presents the ANCOVA results with the average of the assessors' quality ratings of the participants' audit procedure changes, final audit program, and final time budget as dependent variables, the participants' audit risk assessments as covariate, and industry specialization and audit case as independent variables.[14] Panel A of Table 3 reveals that the mean assessors' quality ratings of the industry-matched participants' audit procedure changes, final audit programs, and final time budgets are higher than those of the industry-mismatched participants for both the higher-risk and lower-risk cases. The results also show that the mean assessors' quality ratings are generally higher for the higher-risk case than for the lower-risk case. Panel B of Table 3 indicates that the main effects of industry specialization ($p < 0.016$) and unexpectedly, audit case ($p < 0.000$), are significant for the participants' audit procedure changes, final audit programs, and final time budgets.

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[14] I include the participants' audit risk assessments as covariate because the assessors are provided with the participants' risk assessments. Thus, the participants' risk assessments could have affected the assessors' evaluations. Informing the assessors of the participants' risk assessments is consistent with practice because audit partners and/or managers normally would be cognizant of their subordinates' risk assessments from the audit work paper documentation.

TABLE 3
Effects of Auditors' Industry Specialization on Audit Planning Quality (Raw Ratings)

Panel A: Average Quality Ratings of the Participants' Audit Procedure Changes, Final Audit Programs, and Final Time Budgets (standard deviations in parentheses)[a]

	Industry-Matched Participants			Industry-Mismatched Participants		
	Higher-Risk	**Lower-Risk**	**Combined**	**Higher-Risk**	**Lower-Risk**	**Combined**
Mean Quality Ratings						
Procedure-changes[b]	7.079 (1.042)	4.407 (2.257)	5.934 (2.131)	6.448 (1.406)	4.128 (1.764)	5.272 (1.973)
Staff-changes[c]	7.187 (0.758)	4.586 (1.259)	5.636 (1.676)	6.926 (0.957)	4.217 (1.510)	5.655 (1.839)
Hour-changes[d]	7.096 (0.851)	4.419 (1.265)	5.884 (1.703)	7.029 (0.663)	3.993 (0.906)	5.515 (1.715)
Final audit programs	7.553 (0.643)	4.471 (1.317)	6.097 (1.855)	6.903 (1.440)	3.709 (1.078)	5.307 (2.045)
Final time budgets	7.737 (0.510)	5.294 (1.532)	6.583 (1.654)	7.194 (1.123)	4.065 (1.340)	5.629 (1.998)

Panel B: ANCOVA of the Average Quality Ratings of the Participants' Audit Procedure Changes, Final Audit Programs, and Final Time Budgets

		F-Value (p)				
Source	**Degree of Freedom**	**Procedure-Changes**	**Staff-Changes**	**Hour-Changes**	**Final Audit Programs**	**Final Time Budgets**
Audit risk assessment (covariate)	1	1.572 (0.211)	4.534 (0.034)	3.892 (0.049)	0.011 (0.916)	1.195 (0.277)
Industry specialization	1	8.221 (0.004)	5.855 (0.016)	9.962 (0.002)	7.934 (0.006)	12.42 (0.001)
Audit case	1	231.3 (0.000)	514.8 (0.000)	136.1 (0.000)	157.4 (0.000)	124.02 (0.000)
Industry specialization * Audit case	1	0.306 (0.581)	0.189 (0.664)	1.203 (0.273)	0.026 (0.873)	0.653 (0.421)

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[a] The assessors rated the quality of the participants' audit procedure changes, final audit programs, and final time budgets on an 11-point scale with endpoints labeled as 0 = Not appropriate at all and 10 = and Most appropriate.

[b] Procedure-changes refer to modifications made by the participants to the planned audit procedures such as addition of new procedures, replacement of planned procedures, deletion of planned procedures, and change of sample sizes of planned procedures.

[c] Staff-changes refer to changes made by the participants to the assigned staff level without any change in the planned procedures. The assigned staff level may be substituted with either more or less senior staff level.

[d] Hour-changes refer to changes made by the participants to the budgeted audit hours without any change in the planned procedures and assigned staff level.

As noted in Section III, audit case risk level is confounded with assessor group in the quality-rating task because one group of assessors is assigned to judge the quality of participants' responses to the higher-risk case and another group is assigned to the lower-risk case. To address the issue of audit case risk level being confounded with assessor group, I perform two additional analyses.

One, to minimize assessor-specific impacts, I standardize the individual assessors' quality ratings for the audit procedure changes, the final audit programs, and the final time budgets by converting them into z-scores (Ferguson and Takane 1989, 479–489), and I reperform the earlier ANCOVAs using the average of assessors' standardized quality ratings as dependent variable. The ANCOVA results with the average of assessors' standardized quality ratings as dependent variable reveal only significant main effects of industry specialization ($p < 0.094$, Panel B of Table 4) in the direction predicted by H3. The main effects of audit case are no longer significant ($p > 0.334$, Panel B of Table 4). The results thus support H3.

Two, I perform independent-samples t-tests on the average of assessors' quality ratings within each risk level. Within each risk level, the assessor group is held constant. For the higher-risk case, the means of the assessors' quality ratings are higher for the industry-matched participants' procedure-changes ($p = 0.000$), staff-changes ($p = 0.016$), hour-changes ($p = 0.244$), final audit programs ($p = 0.017$), and final time budgets ($p = 0.012$) than for those of the industry-mismatched participants. For the lower-risk case, the means of the assessors' quality ratings are higher for the industry-matched participants' procedure-changes ($p = 0.170$), staff-changes ($p = 0.021$), hour-changes ($p = 0.003$), final audit programs ($p = 0.026$), and final time budgets ($p = 0.005$) than for those of the industry-mismatched participants.[15] Thus, the results within each audit risk level are also consistent with H3.

V. CONCLUDING COMMENTS

In this study, I seek to contribute to the extant research on auditors' industry experience and/or specialization by investigating whether and how industry specialization affects auditors' performance. More specifically, I examine the effects of auditors' industry specialization on their audit risk assessments and planning decisions as well as the moderating effect of auditors' industry specialization on the sensitivity of auditors' planning decisions to their audit risk assessments.

The study results suggest that auditors' knowledge of the client's industry enables them to better assess the audit risks associated with a client. I find that industry-matched auditors discern differential audit risk levels better than do industry-mismatched auditors, which implies that industry-matched auditors can better identify areas requiring greater audit attention and resources when making audit-planning decisions.

The results also suggest that auditors' knowledge of the client's industry influences their planning decisions in three ways. One, auditors' knowledge of the client's industry directly affects the manner in which auditors modify inherited audit procedures. I find that industry-matched auditors make more procedure-changes than industry-mismatched auditors. On the other hand, industry-mismatched auditors make more hour-changes than procedure-changes. While some reduction of budgeted hours via hour-changes may not affect audit quality, extensive reduction of budgeted hours via hour-changes is likely to increase the time-stress experienced by audit staff during fieldwork and the propensity of

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[15] The results of independent-samples t-tests on the average of the assessors' standardized quality ratings within each risk level are qualitatively similar.

TABLE 4
Effects of Auditors' Industry Specialization on Audit Planning Quality (Standardized Ratings)

Panel A: Average Standardized Quality Ratings of the Participants' Audit Procedure Changes, Final Audit Programs, and Final Time Budgets (standard deviations in parentheses)[a]

	Industry-Matched Participants			Industry-Mismatched Participants		
	Higher-Risk	Lower-Risk	Combined	Higher-Risk	Lower-Risk	Combined
Mean Standardized Ratings						
Procedure-changes[b]	0.132 (0.779)	0.012 (1.082)	0.081 (0.921)	−0.268 (0.998)	−0.084 (0.861)	−0.175 (0.933)
Staff-changes[c]	0.077 (0.575)	0.181 (0.628)	0.139 (0.607)	0.018 (0.653)	0.001 (0.763)	0.010 (0.705)
Hour-changes[d]	0.083 (0.638)	0.093 (0.613)	0.087 (0.625)	−0.019 (0.539)	−0.095 (0.474)	−0.057 (0.509)
Final audit programs	0.141 (0.662)	0.289 (0.878)	0.211 (0.764)	−0.375 (1.074)	−0.170 (0.705)	−0.273 (0.907)
Final time budgets	0.322 (0.519)	0.410 (0.789)	0.364 (0.652)	−0.198 (0.871)	−0.225 (0.708)	−0.211 (0.787)

Panel B: ANCOVA of the Average Standardized Quality Ratings of the Participants' Audit Procedure Changes, Final Audit Programs, and Final Time Budgets

		F-Value (p)				
Source	Degree of Freedom	Procedure-Changes	Staff-Changes	Hour-Changes	Final Audit Programs	Final Time Budgets
Audit risk assessment (covariate)	1	1.042 (0.308)	0.862 (0.354)	1.110 (0.293)	0.295 (0.588)	0.916 (0.341)
Industry specialization	1	7.539 (0.006)	2.822 (0.094)	8.984 (0.003)	7.283 (0.008)	13.408 (0.000)
Audit case	1	0.142 (0.706)	0.454 (0.501)	0.306 (0.580)	0.944 (0.334)	0.038 (0.846)
Industry specialization * Audit case	1	1.357 (0.245)	0.209 (0.648)	0.078 (0.781)	0.006 (0.938)	0.003 (0.956)

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[a] The assessors rated the quality of the participants' audit procedure changes, final audit programs, and final time budgets on an 11-point scale with endpoints labeled as 0 = Not appropriate at all and 10 = Most appropriate. Each assessor's quality ratings of the participants' audit procedure changes, final audit programs, and final time budgets are then standardized (i.e., converted into z-scores).

[b] Procedure-changes refer to modifications made by the participants to the planned audit procedures such as addition of new procedures, replacement of planned procedures, deletion of planned procedures, and change of sample sizes of planned procedures.

[c] Staff-changes refer to changes made by the participants to the assigned staff level without any change in the planned procedures. The assigned staff level may be substituted with either more or less senior staff level.

[d] Hour-changes refer to changes made by the participants to the budgeted audit hours without any change in the planned procedures and assigned staff level.

audit staff to engage in adverse task behaviors like premature sign-off and underreporting of chargeable time (Alderman and Deitrick 1982; Ponemon 1992).

Two, auditors' knowledge of the client's industry not only directly affects the manner in which auditors modify inherited audit procedures, but also the quality of their audit procedure changes. Industry experts in the study judge the quality of the industry-matched participants' audit procedure changes, final audit programs, and final time budgets to be better than those of the industry-mismatched participants based on the impact of the participants' planning decisions on the audit engagement's effectiveness and efficiency.

Three, auditors' knowledge of the client's industry affects the risk-sensitivity of auditors' planning decisions. I find that industry-matched auditors' final budgeted costs are greater for engagements with higher rather than lower underlying audit risks. On the other hand, industry-mismatched auditors' final budgeted costs do not differ significantly for engagements of differential audit risk levels. I also find that the extent of industry-matched auditors' changes to planned audit procedures (except for their hour-changes) is sensitive to their audit risk assessments, while the extent of industry-mismatched auditors' changes to planned audit procedures is not sensitive to their audit risk assessments. These findings, together with the earlier finding on auditors' ability to discern differential audit risk levels, suggest that auditors' knowledge of the client's industry is crucial to their abilities to make more discerning audit risk assessments and risk-sensitive audit-resource allocation decisions. These abilities have important implications for audit efficiency and effectiveness because inappropriate audit risk assessments and resource allocation decisions will result in over- and/or under-auditing.

Another observation is that while neither the industry-matched auditors' nor the industry-mismatched auditors' hour-changes are sensitive to their audit risk assessments, industry experts judge the quality of the industry-matched auditors' hour-changes to be better than that of the industry-mismatched auditors. This suggests that budgeting more (fewer) hours as audit risk increases (decreases) alone is not necessarily appropriate in addressing audit risk. The key is to consider and modify (where necessary) the nature, assigned staff level, and/or budgeted hours of the planned audit procedures to address the perceived audit risk.

The study findings reveal that the industry-matched auditors reduce total costs even for the higher-risk case, which suggests that the industry-matched auditors may have succumbed to the influence of the partner's efficiency concern. However, other evidence from the study indicates that the industry-matched auditors are not merely reducing the budgeted costs without considering the level of audit risk. On average, the industry-matched auditors' total cost reductions for both the higher-risk and lower-risk cases ($4,374 and $6,330, respectively) do not adequately cover the estimated budgetary deficit ($8,697). Furthermore, the industry-matched auditors' total cost reductions are inversely related to the audit case risk levels, which imply that more audit resources are allocated at higher audit risk levels. Finally, the industry-matched auditors' average risk assessment for the higher-risk case is 5.55, which suggests that they assess audit risk to be moderate rather than high. Thus, the industry-matched auditors' total cost reductions in the higher-risk case do not seem to be inconsistent with their risk assessments. Note that the industry-matched auditors assess audit risk prior to becoming aware of the time budget deficit and the partner's efficiency concern.

The study has several limitations. For instance, the participants come from a single then Big 5 firm, and are mainly at the supervising-senior level. The study findings, thus, may be peculiar to the participating firm and the investigated auditor-rank. Further, the audit case is set in a particular industry (banking). Consequently, I am not able to rule out alternative hypotheses. For example, auditors specializing in the banking industry may respond differently to the 65 percent realization percentage used in the case or be more adept to making procedure-changes than auditors specializing in other industries. However, nothing has come to my attention during the development, pretests, and tests of the study that would suggest that the working practices of participating auditors from different industry specializations would cause a difference between industry groups in the experimental task.

In the study, the auditors are provided with a flawed subordinate's risk assessment and a preliminary audit program and time budget. Future research can investigate the difference between industry-matched and industry-mismatched auditors' performance in assessing audit risks and developing audit procedures without the influence of either a flawed subordinate's risk assessment, or a preliminary audit program and time budget.

In addition, the assessors (industry experts) evaluated the participants' responses based on the presented case facts. Consequently, the assessors' quality ratings may be influenced by how they perceive auditors should respond to those task demands rather than how auditors should ideally respond without considering the flawed subordinate's risk assessments, the constrained time budget, and the partner's efficiency concern. The assessors' quality ratings may also be less than objective and biased by potential cues of the participants' own industry specialization (such as the participants' risk assessments). It is also unclear how the assessors weigh the trade-offs between effectiveness and efficiency in their evaluations of the participants' responses. Future research can examine how industry specialists make these decisions and the factors that affect their decisions.

This study mainly focuses on the effects of auditors' industry specialization on auditors' planning decisions. Future research may investigate the effects of auditors' industry specialization on auditors' execution of audit programs or performance of other tasks such as fraud and error identification and detection. Future research may also investigate ways to improve the risk sensitivity of auditors' planning decisions relating to hour-changes.

APPENDIX

Summary of Industry-Specific Risk Information Embedded in the Audit Cases

For the Higher-Risk Case

a. There is a large increase in the current loan balance (11.04 percent) and the increase is mainly in commercial loans.
b. The bank expanded into two new territories where it has not marketed before.
c. The gap between the bank's average loan yield and the one-year Treasury bill yield has been increasing over the past three years.*
d. Delinquent loan rates have been increasing over the past three years, particularly for the larger components of the commercial loans.*
e. Over the past three years, credit losses have been increasing and credit recoveries have been decreasing, resulting in an increasing trend of net credit losses, particularly for the larger components of commercial loans (both in dollars and percentages of average loans outstanding). There is a large increase in provision for credit losses in the current year.*
f. Nonaccrual asset rates have been increasing over the past three years, particularly for the larger components of the commercial loans.*
g. Similarly, loans past due 90 days or more and still accruing interest have been increasing over the past 3 years.*

*The participants have to identify these trends on their own from the financial information provided in the audit case.

For the Lower-Risk Case

a. There is a large increase in the current loan balance (11.04 percent) and the increase is mainly in consumer loans.
b. The bank's operations and its geographical market have not changed.
c. The gap between the bank's average loan yield and the one-year Treasury bill yield has remained relatively stable in the past three years.*
d. Current delinquent loan rates have decreased compared to the prior two years, particularly for the consumer loan components.*
e. Over the past three years, credit losses have remained relatively stable while credit recoveries increase, resulting in lower current net credit losses, particularly for most of the loan components (both in dollars and percentages of average loans outstanding). The percentage change in provision for credit losses is relatively stable over the past three years.*
f. Nonaccrual asset rates have been either relatively stable or decreasing over the past three years, for most of the loan components.*
g. Loans past due 90 days or more and still accruing interest have been relatively stable over the past three years.*

*The participants have to identify these trends on their own from the financial information provided in the audit case.

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Effects of an Attorney's Line of Argument on Accountants' Expert Witness Testimony

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ABSTRACT: I test whether hints at an attorney's line of argument influence accountants' expert witness testimony. In experiment 1, litigation services accountants review evidence adopted from a recent auditor-liability case and decide whether a defendant auditor complied with generally accepted auditing standards. I find that hints posed before the review of evidence result in decisions that are more consistent with the attorney's verdict preference. Motivated by a lack of research on the role of accountants' expert testimony in judges' decisions and by evidence that most auditor-liability cases settle, I find in experiment 2 that the certainty of decisions in an expert's report affects the settlement decisions of experienced lawyers, a proxy for trial judges. These findings show that hints at an attorney's line of argument can influence accountants' decisions about evidence documenting auditing issues, and that the certainty of decisions in an expert's report can affect the decisions of mock trial judges. The study has implications for drafting attorney-expert retention letters, and for training accounting experts and the judiciary about the role of hints in the attorney-expert setting.

Keywords: *expert witness testimony; line of argument; auditor liability.*

Data Availability: *Contact the author.*

I. INTRODUCTION

Under the Rule of Daubert (*Daubert v. Merrill Dow Pharmaceuticals* 1993), extended to accountants in 1999 (*Kumho Tire Co. v. Carmichael* 1999), a trial judge can dismiss an expert witness whose testimony is influenced by side-taking (Vigilante 2000).[1] Dismissal discredits the witness and can effectively exclude him or her from appearing again as an expert, raising a powerful disincentive to compromise objectivity. However, a body of research in accounting reveals that the decisions of professional accountants

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[1] The Rule of Daubert, established in *Daubert v. Merrill Dow Pharmaceuticals* (1993) for scientific testimony, provides that "a trial judge must ensure that any and all expert testimony or evidence admitted is not only relevant but reliable" (Causey 2001, 423). Following *Kumho Tire Co. v. Carmichael* (1999), the Rule of Daubert also applies to nonscientific testimony by "appraisers, economists, *accountants*, financial analysts and other[s testifying about] the value of assets, properties and business interests" (Reilly 1999, 28) (emphasis added).

may be influenced unintentionally. (See Smith and Kida [1990] for a review.) In this study, I prompt litigation services partners and managers with hints at a line of argument that supports an attorney's verdict preference. Consistent with predictions from a family of theories represented by predecisional distortion (e.g., Russo et al. 1996) and by the related but broader motivated reasoning (e.g., Kunda 1990), I test whether hints influence partners' and managers' decisions about a defendant auditor's compliance with generally accepted auditing standards (GAAS). Murphy (2000, 235) reports that hints occur in practice and, for that matter, may not be unexpected, given an attorney's role as advocate. The link between hints and accountants' expert testimony is important, partly because litigation services are a significant market niche among practicing accountants and partly because conflicted testimony violates the Federal Rules of Evidence (U.S. Congress 2002) and affects verdicts (Champagne 1992; Richey 1994).[2]

Expert witnesses in auditor-liability cases typically review audit working papers, produce a report that concludes whether the auditor complied with GAAS, and testify both in a deposition and, barring settlement, at trial. AICPA members retained as expert witnesses are bound by the Code of Professional Conduct, "General Standards" (AICPA 2002b), which requires objectivity, and by Statement on Standards for Consulting Services (SSCS) No. 1, *Definitions and Standards* (AICPA 1992), which requires competence, care, planning, integrity, and opinions supported by relevant data. To assure the appearance of objectivity and secure compatible testimony, an attorney may interview an undisclosed consultant, expose the consultant to relevant evidence, solicit an oral opinion, and retain the consultant as a testifying expert if his or her opinion is compatible with the attorney's verdict preference (Crain et al. 1994). Nothing at law precludes an attorney from retaining credible compatible testimony, raising a suspicion addressed previously in accounting research (Ponemon 1995) of intentional side-taking or client-advocacy. However, this study addresses an issue that may be incremental to side-taking: an accountant's prior knowledge of an attorney's line of argument. Prior knowledge can unfold in practice when, for example, before exposing a consultant to evidence, an attorney hints briefly at arguments that support the attorney's verdict preference (Murphy 2000).

I study the expert-witness setting in experiments, rather than with archival data, because publicly available trial documents do not generally capture evidence about an expert's prior knowledge of an attorney's argument and because I introduce in experiment 2 a control condition that is unlikely in the real world. In experiment 1, partners and managers in the litigation services department of a Big 5 firm review documents adopted from a recent auditor-liability case. One group of participants is prompted with hints at the attorney's line of argument *before* they review evidence and decide whether the auditor complied with GAAS, another group is prompted *after* they review evidence but before they decide, and a third group is not prompted until *after* they decide. I find that hints posed *before* reviewing evidence result in decisions about an auditor's compliance with GAAS that are more consistent with the attorney's verdict preference.

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[2] On litigation services as a market niche, the AICPA (2001, 37) reports that, "Approximately 12% of all AICPA members in public practice either serve as expert witnesses or have expressed an interest in this consulting area. Litigation support services were among the top ten CPA growth niches in the 1990s." The Sarbanes-Oxley Act (U.S. House of Representatives 2002), Section 201, "Services Outside the Scope of Practice of Auditors, Prohibited Activities," prohibits incumbent auditors from performing "legal services and expert services unrelated to the audit" for audit clients, but does not prohibit legal and expert services for nonaudit clients.

In experiment 2, I address a natural sequel to experiment 1: Given that most auditor-liability cases settle (Bajaj et al. 2000), that judges consider experts' reports during settlement proceedings (Yeazell 2000), and that no prior study addresses the effect of accountants' expert testimony on judges' decisions, I test whether experts' reports necessarily drive experienced attorneys, a proxy for trial judges, to make decisions in the direction of the expert's decision. Experiment 2 consists of two parts. In experiment 2A, different partners and managers in the same litigation services department replicate two of the conditions in experiment 1 and draft outlines of key items for an expert's report. Experiment 2A serves two purposes. First, I find that hints at a line of argument posed before reviewing evidence affect decisions made after outlining report items, offering evidence that the influence of hints reported in experiment 1 persists to decisions that follow tasks focused on an expert's report. Second, in a controlled setting, a subset of the participants in experiment 2A use the full set of key-item outlines to help prepare an expert's report for experiment 2B.

In experiment 2B, one group of mock judges reviews evidence and decides *after* reading an expert's report that carries *more certainty* about an auditor's compliance with GAAS, one group reviews evidence and decides *after* reading a report that carries *less certainty*, and one group decides *before* reading a report (control condition). The more certain and less certain reports differ only in the relative degree of certainty carried by each; the more certain report asserts that the auditor "did" gather evidence and that an account was "not" materially misstated, and the less certain report asserts that the auditor "likely did" gather evidence and that an account was "likely not" materially misstated. I find that the decisions of mock judges are in the direction of the experts' decisions, suggesting that the certainty conveyed in experts' reports affect judicial decisions.

I interpret the results of this study as evidence that hints at an attorney's line of argument provided before reviewing evidence can influence accounting experts' decisions about an auditor's compliance with GAAS, and that the degree of certainty in an expert's report can affect mock judges' decisions in settlement rulings. This interpretation has implications for drafting attorney-expert retention letters, and for training accounting experts and the judiciary that predecisional distortion or motivated reasoning can affect expert testimony. For example, although potentially costly, the effect of hints at an attorney's argument can be eliminated at low cost; while not standard practice today, an accountant, before retention as a consultant or as a testifying expert, could include a statement in a retention letter, or announce to the attorney orally, that he or she will forgo entertaining the attorney's argument until after delivering a first-draft report. Clearly, an accounting expert would know whether an attorney represents the plaintiff or the defense, thereby revealing the attorney's verdict preference. However, one of the issues at stake in an expert witness engagement is what the attorney tells the accountant—hints at the attorney's line of argument—not the attorney's verdict preference. This study demonstrates that hints at an attorney's line of argument can influence an expert's decisions over and above any effect that may be attributed to the accountant's knowledge of the attorney's verdict preference.

Although framed in predecisional distortion and motivated reasoning, the expectations posed and the results reported in this paper are not inconsistent with other related theories, among them priming and accountability. Research on priming has repeatedly documented the influence of prior oral or visual representations on recognition memory in general and on decision making in particular. (See Richardsonklavehn and Bjork [1988] for a review.) In turn, research on accountability reports that known preferences affect the views of accountable decision makers. (See Lerner and Tetlock [1999] for a review.) I do not claim that the theories of predecisional distortion and motivated reasoning affect an accounting expert's decision making uniquely or to the exclusion of other related theories. Rather, prior research on predecisional distortion and motivated reasoning present promising experimental manipulations from which to observe accountant behavior across the three alternatives I use to sequence an attorney's argument: before reviewing evidence, after reviewing evidence, and after making a decision.

Section II analyzes relevant literature and develops hypotheses. Sections III and IV report the design and results of experiments 1 and 2, respectively. Section V summarizes and concludes.

II. LITERATURE ANALYSIS AND HYPOTHESES

Prior research on auditor-liability trials focuses largely on juror decision making. For example, there is evidence that jurors assign more credibility to government than to AICPA standards (Buckless and Peace 1993), that jurors hold auditors to higher standards of care when audit failure is more severe (Kadous 2000), and that juror decision making is not independent of the use and reliability of decision aids (Lowe et al. 2002). Juror decision-making research in accounting informs this study for two reasons. First, the issues surrounding each of the findings cited—auditing standards, auditor care, and audit decision aids—would likely be addressed in expert testimony, since each meets the "technical, or other specialized knowledge" standard of Rule 702, "Testimony by Experts," of the Federal Rules of Evidence (U.S. Congress 2002). Second, a number of studies report that expert testimony affects jury verdicts. For example, Champagne (1992) found that 65 percent of surveyed jurors were influenced by expert testimony, and Richey (1994) found that merely referring to a witness as an "expert" afforded the witness credibility. Despite the potential importance of expert testimony to legal proceedings and to juror decision making, and the frequency with which accountants appear as experts, surprisingly little research investigates the behavior of accountants as expert witnesses.

Predecisional Distortion, Motivated Reasoning, and an Attorney's Line of Argument

I propose that the predecisional distortion of evidence, documented both in psychology and in accounting, may influence the certainty of decisions accounting experts make after reviewing evidence and performing expert-witness tasks. Carlson and Russo (2001, 91) define predecisional distortion as "the biased interpretation and evaluation of new information to support whichever alternative is currently leading during a decision process." In the attorney-expert setting, the attorney's line of argument is said to be "leading" because the attorney has been exposed to the evidence but the accounting expert has not. However, in this study, I focus on an institutional detail—hints at an attorney's line of argument—that is incremental to an attorney's verdict preference and potentially influential to an accounting expert's decisions. Therefore, the essential feature of the definition for this study is that "biased interpretation and evaluation" may occur when an expert witness is prompted with hints at an advocate attorney's line of argument *before* reviewing evidence, rather than *after* reviewing evidence. This feature is consistent with motivated reasoning. For example, based in the premise that "motivation may affect reasoning through reliance on a biased set of cognitive processes" (Kunda 1990, 480), research on motivated reasoning demonstrates that directionally motivated decision makers may adopt evaluation strategies likely to produce desired decisions, although the decisions produced are constrained by reasonableness (Boiney et al. 1997).

Research has documented predecisional distortion in restaurant choice (Russo et al. 1996), brand preference (Russo et al. 1998), product information (Meloy 2000), professionals making decisions unrelated to their professions (Russo et al. 2000), juror decision making (Carlson and Russo 2001), and auditor going-concern decisions (Wilks 2002). For example, Wilks (2002) finds that audit managers prompted by a partner's optimistic or pessimistic statement before evaluating evidence sentences make going-concern decisions consistent with the partner's statement. Likewise, research has documented motivated reasoning in autobiographical memory (Sanitioso et al. 1990), statistical heuristics (Sanitioso and Kunda 1991), auditor incentives (Hackenbrack and Nelson 1996), and client preferences in tax research (Cloyd and Spilker 1999), among other things. For example, Cloyd and Spilker (1999) find that tax professionals' searches for judicial precedent are not independent of a client's preferred treatment.

Hints at an attorney's line of argument posed before the review of evidence may affect an accounting expert's decisions for three reasons. First, the effect may be unintentional, thereby offering an accountant little opportunity to avoid the distortion of evidence, absent training. Second, like Wilks (2002), who studied an audit setting using experienced participants (Big 5 managers), I study an audit setting and use experienced participants (Big 5 partners and managers). Third, the evidence in an auditor-liability case is unfamiliar to an accounting expert, thereby suggesting that hints at a line of argument may bear subtly on the accountant's evaluation of the evidence.

In this study, I probe whether the timing of hints at an attorney's line of argument influences differently the certainty in accounting experts' decisions. Consistent with research on predecisional distortion and motivation reasoning, I expect that access to an attorney's line of argument *before* reviewing evidence may promote reliance on biased cognitive processes that distort the interpretation and evaluation of the evidence, thereby influencing the certainty of decisions accountants make about an auditor's compliance with GAAS. Accountants prompted with an attorney's line of argument *before* reviewing evidence and before deciding are likely to be more certain that an auditor complied with GAAS than accountants prompted *after* reviewing evidence and before deciding, since the probative value of the attorney's argument will be available to the accountants who are prompted before they interpret and evaluate each document in evidence. I also study hints at an attorney's line of argument presented *after* decision making, the equivalent of not having been prompted at all. This allows me to mimic the condition from practice in which attorneys do not prompt accountants, thereby providing a benchmark against which to compare the behavior of accountants who *are* prompted with hints either before or after reviewing evidence. I test the following alternative hypothesis:

> **H1:** Accounting expert witnesses prompted with hints at a defense attorney's line of argument *before* reviewing evidence and *before* making a decision are likely to be more certain that an independent auditor complied with GAAS than accounting expert witnesses prompted *after* reviewing evidence and *before* making a decision and accounting expert witnesses prompted *after* making a decision.[3]

Experts' Reports and Trial Judges

Although prior studies report that expert testimony affects jury verdicts (e.g., Reinard 1988; Champagne 1992; Richey 1994), there is reason to redirect auditor-liability research toward the effect of expert testimony on trial judges, a topic not studied previously. Clearly, jury trials are widely available in the U.S., a right conferred with some exceptions by the Seventh Amendment to the U.S. Constitution. (See Yeazell [2000] for a discussion.) However, jury trials in auditor-liability cases are uncommon. For example, based on a sample of federal class action suits filed from 1988 to 1999, Bajaj et al. (2000, 10) conclude that, "Cases involving accounting firms rarely went to trial."[4] This suggests that the larger issue in auditor litigation may be the effect of an expert's decisions on judges who rely, in part, on experts' reports to rule on fairness in settlement hearings.

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[3] To optimize task realism, I study hints prompted by defense attorneys, to the exclusion of plaintiff attorneys, since the participating firm does not accept engagements to testify against auditors.

I study experts' reports, rather than deposition and trial testimony, because a report is the first deliverable in the sequence of tasks an expert performs and the basis for an opposing attorney's cross examination at deposition and at trial. I expect that the certainty of decisions in experts' reports can affect the decisions of judges, since the decisions are part of the evidence set that the judges will evaluate. To the extent that an expert's testimony carries more certainty, a judge's reliance on the expert's decision could drive judicial decisions in the direction of the expert's decision. This expectation is consistent with Guthrie et al. (2001), who find that federal magistrate judges made aware of a Court of Appeals ruling are significantly more likely to make decisions consistent with the Court's ruling than judges unaware of the ruling. I test the following alternative hypothesis:

> **H2:** Judges prompted with an expert's more certain decision *before* reviewing evidence and ruling on an independent auditor's compliance with GAAS are likely to be more certain that the auditor complied than judges prompted with an expert's decision *after* reviewing evidence and ruling.

Hypothesis 2 compares the decisions of judges who read a more certain report before ruling to the decisions of judges who do not. To compare the decisions of judges who read a more certain report before ruling to the decisions of judges who read a less certain report before ruling, I test the following alternative hypothesis:

> **H3:** Judges prompted with an expert's *more certain* decision before reviewing evidence and ruling on an independent auditor's compliance with GAAS are likely to be more certain that the auditor complied than judges prompted with an expert's *less certain* decision before reviewing evidence and ruling.

III. EXPERIMENT 1: EFFECTS OF AN ATTORNEY'S LINE OF ARGUMENT ON ACCOUNTING EXPERTS' DECISIONS

Method

Experiment 1 addresses the research question, Do hints at an attorney's line of argument influence the certainty of accountants' decisions about auditor-liability evidence? The research design is a 1 (*litigation services partners and managers*) × 3 (*line of argument*: known to the accountant *before* reviewing evidence and *before* deciding versus known *after* reviewing evidence and *before* deciding versus known *after* reviewing evidence and *after* deciding) between-subjects experiment.[5] The dependent variable is a 15-point bipolar decision about whether an independent auditor did or did not comply with GAAS.

I arranged and implemented the known *after* reviewing evidence and *before* deciding condition several months after the other two conditions were completed. The condition offers a manipulation consistent with prior accounting research in predecisional distortion (Wilks 2002), but raises a risk that the results are tainted by the time span between the completion of the *before* reviewing and deciding and of the *after* reviewing and *after* deciding conditions and the implementation of the *after* reviewing and *before* deciding condition. All of the participants in the *after* reviewing and *before* deciding condition reported that they did not discuss the study with participants from the other two conditions.

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[4] Most securities class action cases settle, whether related to auditor liability or not. For example, Martin et al. (1996) report for a sample of 998 cases that 802 settled, 178 were dismissed, and only 18 went to trial.

[5] For ease of expression, I use the terms "before" or "after *deciding*." However, the design offers no evidence about when in the decision process participants *reach* a decision. Rather, the design standardizes within experimental conditions when participants *record* a decision.

Participants

Participants in experiment 1 were 14 partners and 31 managers (n = 45) assigned permanently (17), temporarily (12), or previously (16) to the litigation services department of a Big 5 firm.[6] Twenty had filed an expert's report, 19 had given deposition testimony, and 9 had given trial testimony in at least one court proceeding or alternative dispute resolution (e.g., arbitration).[7] All 45 reported experience in assurance services, and none reported that the accounting and auditing issue leading to the GAAS decision task in experiment 1 was unfamiliar to them. The partners ranged in experience from 11 to 22 years (mean = 15 years) and the managers from 5 to 12 years (mean = 8 years). I attempted to equalize across experimental cells the number of partners and managers; the number of participants assigned permanently, temporarily, or previously to litigation services; the number who had filed an expert's report and given deposition and trial testimony; and experience. The results were not affected significantly by any of these control variables. For parsimony, I report pooled data.

Materials

Documents in the experiment were adopted from the audit working papers entered into evidence in a Big 5 auditor-liability case tried to verdict in 1998. The documents were adopted because, compared to other cases litigated about the same time (e.g., *In re Sunbeam Securities Litigations* 1998; *In re Waste Management Securities Litigation* 1997), the case was not highly publicized, thereby offering the advantage of issues that were rich in context but not likely known to the participants. In response to a debriefing question asking whether the participants were familiar with the case (1 = unfamiliar, 5 = familiar), 44 of 45 reported they were not. The participant who was familiar with the case rated his or her familiarity as 3. Dropping the participant's response does not affect the results significantly. I include it in the reported data.

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[6] Owing to the limited number of partners and managers assigned permanently to litigation services and available for the study, the firm included in the participant pool partners and managers who practiced in the department temporarily or previously.

[7] There is no significant difference in participants' experience for any of the pairwise comparisons between the *before/before* (range: 6–20 years; mean = 10.4 years), *after/before* (5–22; 9.4), and *after/after* (6–20; 10.0) conditions ($p \geq 0.17$). However, manipulating the *after/before* condition necessarily demanded that I rely less on partners than on managers, and that the condition be dominated by participants assigned previously to litigation services and by participants who had not filed an expert's report, been deposed, or testified. On partners and managers, there is little difference in the number assigned to the *before/before* (6 and 9, respectively) and the *after/after* (5 and 10, respectively) conditions, but the number assigned in the *after/before* condition was 3 and 12, respectively. On participants assigned previously to litigation services, there is no difference in the number of participants assigned to litigation services permanently, temporarily, or previously in the *before/before* (8, 4, and 3, respectively) and the *after/after* (8, 4, and 3, respectively) conditions, but the number assigned in the *after/before* condition was 1, 4, and 10, respectively. In turn, on participants who had filed a report, been deposed, or testified, there is little difference in the number of participants who had filed an expert's report, been deposed, or testified in an auditor liability case in the *before/before* (9, 7, and 4, respectively) and *after/after* (10, 7, and 4, respectively) conditions, but the number in the *after/before* condition was 1, 5, and 1, respectively.

Operated by one general partner, the company, a Delaware public partnership, solicited investments from limited partners and, with the invested funds, acquired high- and low-technology equipment for lease to unaffiliated third parties. The funds were partitioned by type of equipment (e.g., medical equipment, graphic processing equipment), and the general partner paid each fund's limited partners monthly and quarterly distributions from proceeds remaining after allocation of the general partner's and other expenses to the funds. Plaintiffs' claim arose from an undetected cash embezzlement that unraveled the partnership. Because the embezzlement was not documented in the audit working papers, a partner from the participating firm and I assembled experimental materials from working papers the auditors compiled for three other issues, all of which were significant to the central question in the auditor's due diligence defense: Did the firm comply with GAAS?[8] The three issues were: lease loss reserves (31 documents, 77 working paper pages), the allocation of general partner expenses to individual funds (15 documents, 68 pages), and discussion memos (18 documents, 67 pages). These three issues were optimal for the experiment because both plaintiffs' and defendant's experts testified to all three issues. Appendix B lists for each issue the title (and order) of the documents within the working paper file and the number of pages in each document.

Appendix A, Panel A, summarizes for experiment 1 the sequence of materials by condition. The materials consisted of six packets. Packet 1 explained that the participants represented the defense, and included selected *allegations*, excerpts from the *MD&A* and from the *financial statements*, and the defendant auditor's *unqualified opinion*. Packets 2 (lease loss reserves), 3 (allocation of general partner expenses), and 4 (discussion memos) included the *audit working paper* documents listed in Appendix B. To rule out working paper order effects (Ricchiute 1992), I randomized the order of the documents in packets 2 (listed in Appendix B, Panel A), 3 (Panel B), and 4 (Panel C) differently for each participant in the experiment. Packet 5, the decision page, elicited measures from the participants for:

- *Evidence evaluation*: A rank order of the five documents (1 = highest, to 5) per issue they judged most important to their decision,[9] and
- A *decision* (the main dependent variable): To the question, "Did the auditor comply with the third standard of fieldwork of the AICPA's generally accepted auditing standards (GAAS)?" they responded on a 15-point bipolar scale in which -7 = certain that auditor did not comply, $+7$ = certain that auditor did comply, and 0 = uncertain.[10]

The packet included the text of the third standard of fieldwork (AICPA 2002a, para. 150.02). Packet 6 asked a series of debriefing questions, including a check on the line-of-argument manipulation that asked participants in the argument known *before* (*after*) reviewing evidence and *before* deciding conditions whether they remembered that they had read the attorney's line of argument before (after) reading the defendant firm's audit working papers, and asked participants in the argument known *after* reviewing evidence and *after* deciding condition whether they remembered that they had read the attorney's line of argument after deciding.

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[8] The standard of care in an auditor-liability case is the auditor's compliance with GAAS. Underscoring the importance of GAAS at law, Fife (1998, 24), commenting on *National Medical Transportation Network v. Deloitte & Touche* (1998) says, "The court's decision in Medtrans is significant because it held that an accountant's duty of care to a client can, as a matter of law, be based on professional standards rather than on rules of the law that are contrary to rules promulgated by the AICPA."

[9] Within each of the three conditions—lease loss reserves, allocation of expenses, and discussion memos—rank-order correlations among the individual documents participants ranked most important are significant ($p \leq 0.007$). This result indicates that participants in each condition judged roughly the same documents most important to their decisions. Unlike Wilks (2002), I did not gather diagnostic evidence bearing on the participants' evaluation of individual documents.

[10] The labeling of the decision scale is consistent with Tan (1995) and Wilks (2002), and the scale is consistent with Wilks.

Procedures

I assembled and delivered numbered packets to a coordinating partner, and explained that, to equalize cells, the partner should deliver the packets to the individual participants I had selected, and should reinforce to the participants the written instruction to neither discuss the experiment nor consult resources. The coordinating partner returned completed materials to me in overnight mailings. There is no *a priori* reason to believe the experiment was not carried out faithfully. For example, in an email that introduced the study to the participants, and again in a cover letter that included my instructions, the coordinating partner told the participants that the firm was "inclined to influence firm policy based on the results of the study."

All of the participants (n = 45) received packets 1, 5, and 6 and, to rule out the unintended influence of any of the three issues, one-third of the participants (n = 15; 5 in each of the three conditions) received packet 2 (lease loss reserves), one-third received packet 3 (allocation of general partner expenses), and one-third received packet 4 (discussion memos).[11] To manipulate the argument known *before* reviewing evidence and *before* deciding condition, I included the attorney's line of argument as the first page in packet 1. To manipulate the argument known *after* reviewing evidence and *before* deciding condition, I included the attorney's argument as the first page in packet 5 (decision page). To manipulate the argument known *after* reviewing evidence and *after* deciding condition, I included the page as the first page of packet 6 (the debriefing questionnaire)—that is, the first page of the first packet that followed the participant's decision. To rule out drawing extra attention to the line-of-argument manipulation, I included the line-of-argument page within packets, rather than in a separate packet.

The attorney's line of argument was captured in the following language that was drafted by the coordinating partner, a lawyer in the firm's office of the general counsel, and me, and posed in the materials as a statement made by the defense attorney:

> While assembling documents, the firm's lead defense attorney remarked that "the benchmark to measure audit quality is vague. AU Sec 230, Due Care, just says "observe the standards of field work." How do you measure that? Look at the extent of the work they did—for example, the detailed memos that raised and addressed all critical problems.

Although the quoted language is colloquial, both the coordinating partner and the lawyer in the general counsel's office believed the language was close to how an attorney might hint—briefly and likely without reference to specific documents. The coordinating partner said, "The lawyers won't tell you what to think; they'll tell you what to think *about*"—in this case, vague standards, balanced by adequate work, supported by diagnostic memos, and use of the term "extent," which appears in the second standard of fieldwork (AICPA

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[11] There are no significant differences in the mean responses of participants across the lease loss reserve, allocation of general partner expenses, and discussion memo conditions ($p \geq 0.20$).

2002a, para. 150.02) and speaks to the amount of work performed. To emphasize realism, I did not impose a task that would heighten the participants' awareness of the attorney's argument, a departure from prior research in accounting (Wilks 2002) but consistent with prior research in psychology (e.g., Russo et al. 1996; Carlson and Russo 2001). In response to a debriefing question, all of the participants responded that they read the materials and completed the decision page in packet number order.

Results

All of the participants completed the decision page. As a check on the line-of-argument manipulation, all 15 (15) participants in the argument known *before* (*after*) reviewing evidence and *before* deciding condition remembered correctly that they had read the attorney's line of argument before (after) reading the defendant firm's audit working papers, and all 15 participants in the argument known *after* reviewing evidence and *after* deciding condition remembered correctly that they had read the attorney's line of argument after deciding. The following reports data for all 45 participants in experiment 1.

Table 1 reports descriptive statistics for the participants' decisions (Panel A) and planned contrasts (Panel B).[12] In Table 1 and the following discussion, cell 1 refers to the argument known *before* reviewing evidence and *before* deciding condition, cell 2 to the known *after* reviewing evidence and *before* deciding condition, and cell 3 to the known *after* reviewing evidence and *after* deciding condition. Hypothesis 1 predicts that accounting expert witnesses prompted with hints at a defense attorney's line of argument *before* reviewing evidence and *before* deciding (cell 1) are likely to be more certain that an auditor complied with GAAS than accounting expert witnesses prompted *after* reviewing evidence and *before* deciding (cell 2) and accounting expert witnesses prompted *after* deciding (cell 3). Table 1, Panel B, shows that the participants who knew the attorney's argument *before* reviewing evidence and *before* deciding (cell 1) were significantly more certain that the auditor complied with GAAS than the participants who learned the attorney's argument *after* reviewing evidence and *before* deciding (cell 2) (cell 1 versus cell 2: $t_{28} = 1.784$, $p = 0.043$, one-tailed) and the participants who learned the attorney's argument *after* deciding (cell 3) (cell 1 versus cell 3: $t_{28} = 1.910$, $p = 0.033$, one-tailed).[13] These results support H1.

Table 1, Panel B, also reports one other contrast, unrelated to H1. The contrast shows that there is no significant difference between the decisions of participants who knew the attorney's argument *after* reviewing evidence and *before* deciding (cell 2) and participants who did not know the attorney's argument until *after* deciding (cell 3) (cell 2 versus cell 3: $t_{28} = 0.075$, $p = 0.941$, two-tailed). This suggests that there is no systematic reason to attribute the rank order of cells 2 and 3 to anything other than random chance. Since participants in both cells learned the attorney's line of argument *after* reviewing evidence,

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[12] The highest cell mean I report in experiment 1 (cell 1: 1.73) is comparable to the highest cell means in Wilks (2002) and in Tan (1995), both of which also report 15-point bipolar decisions for experienced Big 5 professionals; Wilks (2002) and Tan (1995) report no cell mean higher than 1.93 and 2.87, respectively.

[13] Although I am aware of no theory that offers unambiguous predictions about whether the size of an evidence set bears on the effects of motivated reasoning, I replicated cells 1 and 3 of experiment 1 in a *multiple-issues* condition with 10 partners and 20 managers ($n = 30$) from the same firm. Ten read the issues in packet 2, followed by packet 3, followed by packet 4; 10 read the issues in 4-2-3 order, and 10 read the issues in 3-4-2 order. Unlike the results reported for experiment 1, I found that the participants who knew the attorney's argument *before* reviewing evidence (cell 1) were *not* significantly more certain that the auditor complied with GAAS than the participants who learned the argument *after* (cell 3) ($t_{28} = 0.242$, $p = 0.812$, two-tailed). Absent theory, I can not explain the multiple-issues result systematically. Among other things, the result could be attributed to information overload (i.e., the evidence set consisted of 64 documents and 212 pages).

TABLE 1
Descriptive Statistics and Planned Contrasts—Experiment 1

Panel A: Mean GAAS Decisions[a] (Standard Deviations) and Sample Size [cell number]

	Attorney's Line of Argument		
	Known before Reviewing Evidence and before Deciding	**Known after Reviewing Evidence and before Deciding**	**Known after Reviewing Evidence and before Deciding**
	[1]	[2]	[3]
mean	1.73	0.27	0.20
s.d.	(1.98)	(2.49)	(2.39)
n	15	15	15

Panel B: Planned Contrasts: Effect of Hints at a Line of Argument

	t-statistic	**p-value**
H_1: Known *before* reviewing evidence and *before* deciding v. *after* reviewing evidence and *before* deciding Cell 1 v. Cell 2	1.784	0.043*
H_1: Known *before* reviewing evidence and *before* deciding v. *after* reviewing evidence and *after* deciding Cell 1 v. Cell 3	1.910	0.033*
Other Contrast Known *after* reviewing evidence and *before* deciding v. *after* reviewing evidence and *after* deciding Cell 2 v. Cell 3	0.075	0.941**

*,** p-value is one-tailed, two-tailed, respectively.

[a] Participants indicate on a 15-point bipolar scale whether the auditor complied with the third standard of fieldwork (−7 = certain that auditor did not comply, +7 = certain that auditor did comply, and 0 = uncertain).

this result, combined with the results for the planned contrasts, shows that the differences in the certainty of decisions across conditions were driven by sequencing the hint in cell 1 *before* the review of evidence; sequencing the hint *after* the review of evidence (cell 2) versus *after* deciding (cell 3) did not affect significantly the participants' decisions in cells 2 and 3. However, statistically insignificant differences in the certainty of decisions between cells 2 and 3 do not necessarily translate to insignificant differences in information processing. Differences in processing are more likely transparent in data on the evaluation of individual documents than in data on the certainty of decisions.

Inspection of the cell means in Table 1, Panel A, shows that, although the observed mean in the argument known *before* reviewing evidence and *before* deciding condition (cell 1: 1.73) was higher than the mean in either of the other two conditions (cells 2 and 3: 0.27 and 0.20, respectively), the participants on average in each condition made decisions that centered above 0 (uncertain) but closer to 0 than to +7 (certain the auditor complied) on the 15-point bipolar decision scale. That is, participants in the argument known *before* reviewing evidence and *before* deciding condition were significantly "more certain" than participants in the other two conditions, but certainly were not what might loosely be called "highly" certain (i.e., not near +7) that the auditor complied with GAAS. This result suggests that the materials, adopted from an auditor-liability case in which plaintiffs' and defendant's experts concluded differently, carried sufficient uncertainty to make the participants' decisions close calls, a setting well suited for this study.

IV. EXPERIMENT 2: EFFECTS OF THE CERTAINTY IN EXPERTS' REPORTS ON TRIAL JUDGES' DECISIONS

The key finding of experiment 1 is that hints at an attorney's line of argument posed to accountants before the review of evidence result in decisions about an auditor's compliance with GAAS that are more consistent with the attorney's verdict preference. This finding sets up line of argument as a fruitful platform from which to probe further the influence of hints on accountants' expert witness testimony. In experiment 2, I test whether the effect of hints on an expert's report necessarily affects the decisions of judges in auditor-liability cases. In short, do hints at an attorney's line of argument matter to the outcome of a judicial proceeding? I frame the experimental setting as a trial judge making a settlement-hearing decision about a defendant auditor's compliance with GAAS, a decision judges make commonly from working papers and from experts' reports. To prepare materials for the experiment, I first replicate two conditions from experiment 1 in a new experiment (2A) that, unlike experiment 1, adds an additional task: Partners and managers outline report items I use to prepare an expert's-report excerpt for experiment 2B, which tests H2 and H3. I report in succession the design and results of experiments 2A and 2B, respectively.

Experiment 2A

The design and dependent variable in experiment 2A are identical to two conditions in experiment 1: a 1 (*litigation services partners and managers*) × 2 (*line of argument*: known to the accountant *before* reviewing evidence and before deciding versus known after reviewing evidence and *after* deciding) between-subjects experiment. The dependent variable is a 15-point bipolar decision about whether an independent auditor did or did not comply with GAAS. The two line-of-argument conditions replicate cells 1 and 3 from experiment 1.[14]

Participants

Participants in experiment 2A were 8 partners and 22 managers (n = 30) assigned permanently (15), temporarily (8), or previously (7) to the litigation services department of a Big 5 firm. None participated in experiment 1. Seventeen had filed an expert's report, 12 had given deposition testimony, and 7 had given trial testimony in at least one court proceeding or alternative dispute resolution (e.g., arbitration). All 30 reported prior experience in assurance services, and none reported that the accounting and auditing issue leading to their GAAS decision in the experiment was unfamiliar to them. The partners ranged in experience from 12 to 19 years (mean = 13 years) and the managers from 5 to 10 years (mean = 7 years). I attempted to equalize across experimental cells the number of partners and managers; the number of participants assigned permanently, temporarily, or previously to litigation services; the number who had filed an expert's report and given deposition and trial testimony; and experience. There is no significant difference in the experience of participants across conditions ($p \geq 0.20$) and the results were not affected significantly by any of the control variables. For parsimony, I report pooled data. In response to a debriefing question asking whether the participants were familiar with the case (1 = unfamiliar, 5 = familiar), 28 of the 30 reported they were not. Neither of the two who were familiar with the case rated their familiarity higher than 3. Dropping the two does not affect the results significantly. I include them in the reported data. All of the participants reported that they did not discuss the study with accountants who participated in experiment 1.

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[14] Owing to limitations on the number of participants available for experiment 2A, my design uses the argument known *after* reviewing evidence and *after* deciding condition (rather than the argument known *after* reviewing evidence and *before* deciding condition). I use the *after-after* condition for two reasons. First, the participating firm was most interested in comparing the reports of participants who had been prompted *before* evaluating evidence with the reports of participants prompted *after* evaluating evidence since, in the firm's view, attorney's prompt an expert before the evaluation of evidence or not at all. Second, in experiment 1, there is no significant difference in the certainty of participants in the *after-before* condition and the *after-after* condition (Table 1, Panel B: Other Contrast).

Materials and Procedures

Appendix A, Panel A, summarizes for experiment 2A the sequence of materials by condition. The materials included the packets from experiment 1. However, for experiment 2A, a packet was inserted *before* packet 5 (the decision page)—hereafter the "before 5" packet. Like experiment 1, the packet included an evidence-evaluation task but, unlike experiment 1, included a task to gather data for an expert's-report excerpt. The "before 5" packet elicited from each participant an *outline of ten key items for a report.* The purpose of preparing outlines was to evoke from the participants, before they cast a decision, behavior that was natural to preparing reports. For example, in practice, expert witnesses commonly outline key items before drafting an expert's report. The task allowed the materials to prompt what mattered in experiment 2A: the effect of hints at an attorney's line of argument on the certainty in accounting experts' decisions when preparing reports. I use the outlines to prepare materials for experiment 2B. Line of argument was manipulated as in experiment 1.

All of the participants ($n = 30$) received packets 1, 5, before 5, and 6, and, to rule out the unintended influence of any of the three issues, one-third of the participants ($n = 10$) received packet 2 (lease loss reserves), one-third received packet 3 (allocation of general partner expenses), and one-third received packet 4 (discussion memos).[15] Like experiment 1, I randomized document order (Appendix B) to rule out working paper order effects. I assembled and delivered numbered packets to a coordinating partner, who returned completed materials to me in an overnight mailing. Participants were instructed to complete the experiment unaided in a single uninterrupted sitting, and not to discuss the experiment and not to consult resources. In response to a debriefing question, all of the participants reported that they read the materials in packet number order.

Results

All of the participants completed the decision page. As a check on the line-of-argument manipulation, all 15 participants in the argument known *before* reviewing evidence condition remembered correctly that they had read the attorney's line of argument before reading the defendant firm's audit working papers, and all 15 participants in the argument known *after* deciding condition remembered correctly that they had read the attorney's line of argument after deciding. The following reports data for all 30 participants in experiment 2A.

Table 2 reports descriptive statistics (Panel A) and a planned contrast (Panel B). Table 2, Panel B, shows that the participants who knew the attorney's argument *before* reviewing evidence were significantly more certain that the auditor complied with GAAS than the participants who learned the attorney's argument *after* deciding ($t_{28} = 1.906$, $p = 0.033$, one-tailed). This result shows that the effect reported in experiment 1 for decisions that

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[15] There are no significant differences in the mean responses of participants across the lease loss reserve, allocation of general partner expenses, and discussion memo conditions ($p > 0.20$). Note that this result may be attributable to a lack of power, since sample size = 10.

TABLE 2
Descriptive Statistics and Planned Contrast—Experiment 2A

Panel A: Mean GAAS Decisions[a] (Standard Deviations) and Sample Size

	Attorney's Line of Argument	
	Known before Reviewing Evidence and before Deciding	Known after Reviewing Evidence and after Deciding
mean	2.40	0.73
s.d.	(2.16)	(2.60)
n	15	15

Panel B: Planned Contrast: Effect of Hints at a Line of Argument

	t-statistic	p-value
Known before v. after	1.906	0.033*

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* p-value is one-tailed.
[a] Participants indicate on a 15-point bipolar scale whether the auditor complied with the third standard of fieldwork (-7 = certain that auditor did not comply, $+7$ = certain that auditor did comply, and 0 = uncertain).

follow evidence evaluation persists to decisions accountants make after outlining report items.

Preparation of Expert's Report Excerpt

The expert's report excerpt for experiment 2B was drafted from the outlines that the five accountants in the experiment 2A argument known *before* reviewing evidence condition and the five accountants in the experiment 2A argument known *after* deciding condition prepared for one issue: lease loss reserves. I use one issue only because, in experiments 1 and 2A, there are no significant differences in the mean responses of participants across the lease loss reserves, allocation of general partner expenses, and discussion memo conditions ($p > 0.20$). I selected lease loss reserves because a coordinating attorney and I expected that, at the margin, the participating mock judges in experiment 2B were more likely to understand without experimental intervention (e.g., a tutorial) the economic intuition underlying leases (and reserves for potentially uncollectible leases) than the allocation of general partner expenses (e.g., why allocate?) or the discussion memos (e.g., what are organization and offering costs [Appendix B, Panel C: document 1]?).

I used three steps to prepare an expert's report excerpt. First, following completion of experiment 2A, the five accountants from the experiment 2A known *before* reviewing evidence condition and, separately, the five from the known *after* deciding condition gathered in a room and drafted an excerpt from the key-item outlines they prepared in experiment 2A. This step resulted in *two separate excerpts*, one from the accountants in the known *before* condition and one from the accountants in the known *after* condition. Second, the coordinating partner and an experienced litigator prepared from the two separate excerpts *one composite excerpt* that incorporated common features from the two separate excerpts but also identified the key difference in each separate excerpt's conclusions. The partner and the litigator agreed that the key difference between the conclusions in the two separate excerpts was the degree of certainty the excerpts conveyed about the auditor's gathering, evaluation, and documenting of evidence, and about the material misstatement of lease loss reserves. They captured the relatively *more* certain and the relatively *less* certain conclusions in the following language:

- Relatively *more* certain conclusion: The auditor "**did** gather, evaluate, and document evidence" and the lease loss reserves were "**not** materially misstated" versus
- Relatively *less* certain conclusion: The auditor "**likely did** gather, evaluate, and document evidence" and the lease loss reserves were "**likely not** materially misstated."[16]

Third, I, the coordinating partner, and the litigator edited the composite excerpt into a final excerpt (e.g., converted passive to active voice), reproduced in Appendix C.[17]

Notice in Appendix C that the excerpt uses language (i.e., "did gather," "not materially misstated;" "likely did gather," "likely not materially misstated") consistent with the message that, on balance, the expert's decision is > 0 on the 15-point bipolar scale. Set up this way, the experimental manipulation in experiment 2B depends on the mock judges' interpretation of the degree of certainty in the report excerpt's conclusions.

Experiment 2B

Experiment 2B addresses the research question, Does the certainty in experts' reports affect mock trial judges' decisions about auditor liability? The research design is a 1 (*mock trial judges*) × 3 (*expert's report excerpt*: relatively *more* certain and read by mock judges *before* reviewing evidence versus relatively *less* certain and read by mock judges *before* reviewing evidence versus excerpt read by mock judges *after* deciding) between-subjects experiment. Like experiments 1 and 2A, the dependent variable is a 15-point bipolar decision about whether an independent auditor did or did not comply with GAAS.

Participants

Participants in experiment 2B were 54 experienced attorneys in a law firm, all of whom had trial experience in commercial litigation. The attorneys were partners (48) or of-counsel (6), and ranged in experience from 9 to 24 years (mean = 16 years). Experienced attorneys are a reasonable proxy for trial judges, since judges are attorneys first; the decision task captures evidence that is common in commercial disputes; and, albeit early in their careers, 29 of the participants served in judicial clerkships.[18] In response to a debriefing question asking the participants' familiarity with the case (1 = unfamiliar, 5 = familiar), all 54 reported they were unfamiliar with the case.

Materials and Procedures

Appendix A, Panel B, summarizes for experiment 2B the sequence of materials by condition. Packet 1 included an explanation that the participants assume they are federal district court judges ruling in a *settlement hearing* and, identical to experiment 1, included selected *allegations*, excerpts from the *MD&A* and from the *financial statements*, and the defendant auditor's *unqualified opinion*. Packet 2 included the documents for lease loss reserves. To rule out working paper order effects, I randomized the order of the documents in packet 2 (listed in Appendix B, Panel A) differently for each participant.

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[16] In the auditor-liability case from which the materials were adopted, defendant's expert report used the language "likely did gather, evaluate, and document evidence" to conclude about GAAS and "not likely materially misstated" to conclude about lease loss reserves.

[17] Faced with a design choice, I chose to manipulate the degree of certainty in an otherwise like-worded report excerpt, rather than to manipulate two differently worded report excerpts. The choice was driven by two considerations. First, using differently worded reports presents a potential content confound (i.e., the amount of information between treatments) because the excerpt drafted by the argument known *before* reviewing evidence participants in experiment 2A was longer than the excerpt drafted by the argument known *after* deciding participants. Second, using like-worded reports drafted in final form by the coordinating partner, the coordinating litigator, and me presents a more challenging test because the effects of line-of-argument are vested in the difference between "did" and "likely did" and between "not materially misstated" and "likely not materially misstated."

[18] There is no significance difference between the mean responses of participants who had clerked and participants who had not ($p > 0.20$).

Packet 3 captured the experimental manipulation: an expert's report excerpt about lease loss reserves (Appendix C). Packet 4, the decision page, elicited the main dependent variable (identical to experiments 1 and 2A): The participants were asked to rule on one aspect of the settlement hearing. To the question, "Did the auditor comply with the third standard of fieldwork of the AICPA's generally accepted auditing standards (GAAS)," they responded on a 15-point bipolar scale in which -7 = certain that auditor did not comply, $+7$ = certain that auditor did comply, and 0 = uncertain. The packet included the text of the third standard of fieldwork (AICPA 2002a, para. 150.02). Packet 5 asked a series of debriefing questions, including a question that asked participants who read an excerpt before (or after) making a decision whether they remembered that they did.

Participants in the two key experimental conditions read Packet 3, the expert's-report excerpt, before Packet 4, the decision page. In contrast, participants in the control condition read one of the two excerpts (equalized and randomized) *after* deciding (included in Packet 5, the debriefing questionnaire), the equivalent of not having read an excerpt at all. The purpose of the control condition is to impose a benchmark that is unaffected by an expert's report.

I assembled and delivered numbered packets of experimental materials to the coordinating attorney, and explained that, to equalize cells, the attorney should deliver the packets to the individual participants I had selected. The coordinating attorney returned completed materials to me in one overnight mailing. There is no *a priori* reason to believe the experiment was not carried out faithfully.

Results

All of the participants completed the decision page, and all 36 (18) of the participants who read an expert's-report excerpt *before* (*after*) deciding reported that they did. The following reports data for all 54 participants in experiment 2B.

Table 3 reports descriptive statistics (Panel A) and planned contrasts (Panel B) for the hypothesized effects of an expert's-report excerpt on mock trial judges' decisions. In Table 3 and the following discussion, cell 1 refers to the relatively *more certain* excerpt, cell 2 to the relatively *less certain* excerpt, and cell 3 to the control condition. I use one planned contrast each to test H2 and H3.

Hypothesis 2 predicts that judges prompted with an expert's more certain decision *before* reviewing evidence and ruling on an independent auditor's compliance with GAAS (cell 1) are likely to be more certain that the auditor complied than judges prompted with an expert's decision *after* (cell 3: control condition). In Table 3, Panel A, the mock trial judges' decisions are in the direction of the accounting experts' decisions in experiment 2A. That is, in experiment 2A, the mean response for accountants in the *argument known before* condition (Table 2, Panel A: 2.40) is significantly higher than the mean response for accountants in the *argument known after* condition (0.73). Likewise, in experiment 2B, mock judges who read a relatively *more certain* excerpt before ruling (Table 3, cell 1) are significantly more certain that the auditor complied with GAAS than mock judges in the control condition who did not read an excerpt until after ruling (Table 3, cell 3) (cell 1 versus cell 3: $t_{34} = 3.881$, $p = 0.001$, one-tailed). This result supports H2.

TABLE 3
Descriptive Statistics and Planned Contrasts—Experiment 2B

Panel A: Mean GAAS Decisions[a] (Standard Deviations) and Sample Size [cell number]

	Relatively More Certain Excerpt/Read by Judges before Deciding	Relatively Less Certain Excerpt/Read by Judges before Deciding	Excerpt Read by Judges after Deciding
	[1]	[2]	[3]
mean	4.17	1.89	0.17
s.d.	(2.98)	(2.19)	(3.20)
n	18	18	18

Panel B: Planned Contrasts: Effects of Expert's Report Excerpt

	t-statistic	p-value
H_2: Excerpt read by judges before versus after deciding Cell 1 v. Cell 3	3.881	0.001*
H_3: More versus less certain excerpt read by judges before deciding Cell 1 v. Cell 2	2.614	0.007*

* p-value is one-tailed.

[a] Participants indicate on a 15-point bipolar scale whether the auditor complied with the third standard of fieldwork (−7 = certain that auditor did not comply, +7 = certain that auditor did comply, and 0 = uncertain).

Hypothesis 3 predicts that judges prompted with an expert's *more certain* decision before reviewing evidence and ruling on an independent auditor's compliance with GAAS (cell 1) are likely to be more certain that the auditor complied with GAAS than judges prompted with an expert's *less certain* decision before reviewing evidence and ruling (cell 2). Table 3, Panel B, shows that mock judges who read the relatively *more certain* excerpt before ruling are significantly more certain that the auditor complied with GAAS than mock judges who read the relatively *less certain* excerpt before ruling (cell 1 versus cell 2: $t_{34} = 2.614$, $p = 0.007$, one-tailed). This result supports H3. Together, the results for H2 and H3 show that an excerpt distinguished only by the degree of an expert's certainty can affect mock judges' decisions both when the benchmark is a condition unlikely in practice[19] (H2: cell 1 versus cell 3) and when the benchmark is an expert's report excerpt that carries less certainty (H3: cell 1 versus 2).[20]

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[19] In the real world, trial judges routinely have access to expert testimony *before* ruling. In fact, Causey (2001, 429) reports that auditor-liability cases have been dismissed because litigants did not produce expert testimony to establish a standard of care. For example, in *Battenfeld of America Holding Co., v. Baird, Kurtz & Dobson* (1999), the trial judge found in favor of the plaintiff because Baird, Kurtz & Dobson did not "come forward with any expert testimony with respect to the standard of care for due diligence in connection with a corporate acquisition."

[20] The literature offered reason to expect (and the results of experiment 2B confirm) that the mean response for participants who, *before* deciding, read a relatively *more certain* excerpt (cell 1) would be (is) *greater* than the mean response of the control condition (cell 3) and the other experimental condition (cell 2). However, the literature offered no basis for an expectation about the rank order of mean responses in cells 2 (Table 3, Panel A: 1.89) and 3 (0.17) and, assuming a critical p-value of 0.05, there is no significant difference in the mean responses for the two cells (not reported in Table 3: cell 2 versus cell 3: $t_{34} = 1.882$, $p = 0.068$, two-tailed). This result suggests that there is no systematic reason to attribute the rank order of cells 2 and 3 to anything other than random chance.

V. SUMMARY AND DISCUSSION

This study investigates the links among an attorney's line of argument, expert witness testimony, and the outcome of an auditor-liability case. Consistent with predictions from a family of theories represented by predecisional distortion and motivated reasoning, I show that hints at an attorney's line of argument provided before the review of evidence can influence the certainty of decisions accounting experts make about an auditor's compliance with GAAS (experiment 1), that the effect persists to decisions accountants make after outlining report items (experiment 2A), and that reports distinguished only by the degree of certainty in an expert's conclusion can affect the decisions of mock judges in settlement rulings (experiment 2B). I interpret these results as evidence that hints at an attorney's line of argument can directly influence an accounting expert's decisions, and can indirectly affect a mock judge's settlement rulings. My design allows no degrees of freedom to judge unambiguously whether the effect is intentional or unintentional.

The results of this study have implications for research, for practice, and for education. For research, the findings show that an advocate attorney's prompts may have a considerable effect on accounting experts' decision making, since hints, posed briefly in less than 60 words, drove behavior for a comparatively large evidence set (Appendix B). This suggests that features of predecisional distortion and motivated reasoning may play a role in understanding the complex professional decision behavior of expert witnesses. For practice, I propose a low-cost solution: a statement in a retention letter, or an oral understanding, that the accountant will defer entertaining the attorney's line of argument until after delivering a first-draft expert's report. For education, the study suggests that firm-wide litigation-services training should caution accountants—and defendants' bar should caution litigators—that knowledge of an attorney's line of argument can likely affect expert testimony.

Experiment 1 shows that hints at an attorney's line of argument posed before the review of evidence can influence the certainty in the decisions of accountants who review evidence to prepare expert testimony. This result extends prior research by demonstrating that the effect of experimental manipulations common in studies of predecisional distortion and motivated reasoning are robust not only to expert witness settings, but also to comparatively large evidence sets. For example, whereas my evidence sets varied from 15 to 31 documents and 67 to 77 pages (Appendix B), Wilks' (2002) evidence set consisted of 20 sentences. In experiment 1, the hint captured succinctly an attorney's line of argument, but not the documents the attorney planned to enter into evidence. This raises the question: What features of hints are important to their effectiveness? Would a different hint have driven the results reported in experiment 1? Although an empirical question, I speculate that the effectiveness of a hint, both in experimental research and in practice, is not independent of the trade-off between expert testimony that displays uncompromising objectivity and testimony that displays a witness who has been lead. For example, testimony that reveals that an expert relied largely on documents an attorney flagged is more likely to be interpreted as the latter, than testimony that the attorney did not flag documents.

Experiment 2 shows that a relatively more certain decision in an expert's report excerpt can drive a mock trial judge's decision in the direction of the expert's decision. This finding reveals that the effect of hints on an expert's behavior matters to judicial decisions. However, there is reason to believe that an opposing expert's report could balance the effect. The experiment provides evidence that decisions that differ only in degree of certainty can drive mock judges' decisions in the direction of the more certain report, but the experiment does not address the effect of directionally different reports (i.e., plaintiff *and* defendant attorneys hinting at opposite lines of argument). Generalizations to practice from experiment 2B should be made with caution, since the mock judges did not read report excerpts drafted exclusively by the participating experiment 2A accountants. Rather, the mock judges read an excerpt that was drafted largely by experiment 2A accountants, but edited by three persons who did not participate in experiment 2A and who captured the experimental manipulation in only two words (Appendix C).

The results of this study raise questions for research. I suggest two. First, I study the timing of hints at an attorney's line of argument on the certainty in accounting expert witnesses' *decisions*, and speculate that differences in certainty across experimental conditions may reflect differences in processing. Unlike Wilks (2002), whose participants evaluated each of 20 evidence sentences, participants in my experiments did not evaluate individual documents—a task that would have added significantly to the time demands of the experiment. This raises the question: How does the timing of hints bear on the way that expert witnesses process evidence? Although externally valid replications of the expert-witness setting would likely demand voluminous evidence, time demands could be limited by restricting the setting to an expert engaged to testify on a single, narrow, industry-specific issue, such as reserves for long-duration insurance contracts or accounting for area franchise fees.

Second, I study the certainty of decisions in *written* expert's reports. This raises the question: How do hints bear on an accounting expert's behavior when articulating orally at deposition or at trial the full extent of the detail that supports his or her decisions? The literature offers no theoretical reason to expect that the effects reported in this study do not extend to oral testimony. However, there is reason to believe that depositions and trials impose a tension not present when an expert reviews evidence and drafts a report. For example, at trial, testifying experts face disincentives to delivering testimony that is inconsistent either with their report or with their deposition: cross-examination and impeachment in a public forum. In short, the conscious threat of impeachment may mitigate the effect of hints. The issue awaits research in a setting that can disentangle the effects of conscious and unconscious behavior. Owing to the participating firm's policy of testifying for, but not against, auditing firms, the experimental materials set up the participants to represent the defense. Although I have no reason to believe that hints interact with an attorney's position (plaintiff versus defense), extrapolation of my findings to plaintiff settings may be inappropriate, absent further research.

APPENDIX A
Summary of Materials by Experiment

Panel A: Experiments 1 and 2A

	Experimental Condition			
	Attorney's Line of Argument Known			
Packet	**Before** Reviewing Evidence and **Before** Deciding[a,b]	**After** Reviewing Evidence and **Before** Deciding[a]	**After** Reviewing Evidence and **After** Deciding[a,b]	Example/Reference
1	Plaintiffs' allegations, MD&A and financial statement excerpts, Unqualified opinion **1st page: Attorney's argument**	Plaintiffs' allegations, MD&A and financial statement excerpts, Unqualified opinion	Plaintiffs' allegations, MD&A and financial statement excerpts, Unqualified opinion	Examples: Allegations excerpt: The auditor "represented that its audits had been made in accordance with GAAS. This statement was false and misleading." MD&A excerpt: "demand for Partnership units required the Partnership to close the public offering."
2[c]	Lease loss reserve documents	Lease loss reserve documents	Lease loss reserve documents	Appendix B, Panel A
3[c]	Allocation of partner expenses documents	Allocation of partner expenses documents	Allocation of partner expenses documents	Appendix B, Panel B
4[c]	Discussion memo documents	Discussion memo documents	Discussion memo documents	Appendix B, Panel C
5	Decision page	Decision page **1st page: Attorney's argument**	Decision page	To the question, "Did the auditor comply with the third standard of fieldwork of the AICPA's generally accepted auditing standards (GAAS)," participants responded on a 15-point bipolar scale in which −7 = certain that auditor did not comply, +7 = certain that auditor did comply, and 0 = uncertain.
6	Debriefing questions	Debriefing questions	Debriefing questions **1st page: Attorney's argument**	Example: "Did you read the materials and complete the decision page in packet number order?"

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[a] Conditions in experiment 1.
[b] Conditions in experiment 2A.
[c] One-third of the participants read packet 2, one-third read packet 3, and one-third read packet 4.

APPENDIX A
(continued)

Panel B: Experiment 2B

Packet	Experimental Condition			
	Relatively **More** Certain Excerpt/ Read by Judges **Before** Deciding	Relatively **Less** Certain Excerpt/ Read by Judges **Before** Deciding	Excerpt Read by Judges **After** Deciding	Example/Reference
1	Plaintiffs' allegations, MD&A and financial statement excerpts, Unqualified opinion	Plaintiffs' allegations, MD&A and financial statement excerpts, Unqualified opinion	Plaintiffs' allegations, MD&A and financial statement excerpts, Unqualified opinion	Examples: Allegations excerpt: The auditor "represented that its audits had been made in accordance with GAAS. This statement was false and misleading." MD&A excerpt: "demand for Partnership units required the Partnership to close the public offering."
2	Lease loss reserve documents	Lease loss reserve documents	Lease loss reserve documents	Appendix B, Panel A
3	**Relatively *more* certain excerpt**	**Relatively *less* certain excerpt**	—	Appendix C
4	Decision page	Decision page	Decision page	To the question, "Did the auditor comply with the third standard of fieldwork of the AICPA's generally accepted auditing standards (GAAS)," participants responded on a 15-point bipolar scale in which −7 = certain that auditor did not comply, +7 = certain that auditor did comply, and 0 = uncertain.
5	Debriefing questions	Debriefing questions	Debriefing questions **Includes excerpt**	Example: "Did you read the materials and complete the decision page in packet number order?"

APPENDIX B
Audit Working Paper Documents

	Document	Number of Pages
Panel A: Lease Loss Reserves		
1	Working Paper File Review	1
2	Year-End Conclusion	3
3	Memo re: Review of Client Reserve Analysis	4
4	Reserve Analysis	4
5	Analysis of Credit Reserves in the Financial Industry	1
6	Proposed Internal Control Recommendations	2
7	Credit Reserve Analysis: Fund XVI	9
8	Deficiency Report: Calculation of Aggregate Exposure	1
9	Client's Recommended Reserve for Deficiency	1
10	Explanation of Tick Marks Used	6
11	Credit Reserve Analysis: Fund XVII	2
12	Purchase Accounting	1
13	Purchase Accounting Adjustment	1
14	Explanation of Tick Marks Used	3
15	Frozen Deficiency Report	6
16	Miscellaneous Calculations	1
17	Explanation of Tick Marks	3
18	Credit Reserve Analysis: Fund XVIII	3
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20	Explanation of Tick Marks	1
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22	Memo re: Mathematical Accuracy of Aging Report	1
23	Explanation of Tick Marks	1
24	Credit Loss Reserve Analysis: Miscellaneous Calculations	1
25	Delinquency Aging	4
26	FCLC Portfolio	1
27	Accounts Receivable	2
28	Accounts Receivable Aging Report	2
29	Credit Loss Reserve: Tick Mark Explanations	3
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3	Memo re: General Partner Expense Allocation	2
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APPENDIX C

Expert's Report Excerpt (Manipulations Bolded)—Experiment 2B

Generally accepted accounting principles require that an investment in direct financing leases be carried on the balance sheet at the net realizable value—i.e., minimum lease payments receivable less an allowance for lease losses.

As required by generally accepted auditing standards and the firm's Audit Reference Manual, the working papers include evidence that the auditor **did (likely did)** gather, evaluate, and document evidence supporting all five of management's financial statement assertions about leases—existence or occurrence, completeness, rights and obligations, valuation or allocation, and presentation and disclosure. For example, File 2, Wallet 2, Bundle 4, Lease Loss Review, pages CR4-7 summarize the auditor's audit procedures, reconciled below to related financial statement assertions:

Procedures	WP Page	Assertion(s)
Analytical Review (trend analysis)	CR10	Completeness
Credit file reviews (n = 40 files)	CR50,65,75	Valuation, Existence, Rights
Detailed testing: deficiency reports	CR51,69,81	Valuation, Rights
Detailed testing: lease receivable agings	CR140	Valuation

The auditor's procedures resulted in material adjustments to the lease loss reserve. For example, CR65 cites an adjusting journal entry increasing Bad Debt Expense (and, therefore, reducing reported net income) and increasing the Allowance for Lease Losses by $1,330,000.

Based on evidence within the working papers, I conclude that lease loss reserves were **not (not likely)** materially misstated in the financial statements.

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	Number of Manuscripts	Percent	Cumulative Number	Cumulative Percent
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61 ≤ Days ≤ 90	139	35.46	349	89.03
91 ≤ Days ≤ 120	36	9.18	385	98.21
121 ≤ Days	7	1.79	392	100.00
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1992	241
1993	234
1994	231
1995	195
1996	230
1997	195
1998	196
1999	239
2000	260
2001	260*
2002	324
2003 (8 mths)	217

* 2001—in addition to the 260 regular submissions, 68 MSs were submitted to the *TAR* Quality of Earnings Conference.

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106 INDIANA UNIVERSITY, Department of Accounting, invites applications for two tenure-track faculty positions effective Fall 2004. Preference will be given to Assistant Professors with teaching and research interests in Financial Accounting and Tax, but individuals at all ranks are welcome to apply. Candidates must possess a doctoral degree and have a strong commitment to excellence in scholarly research and teaching. Compensation levels are competitive and negotiable. Interested candidates should send a current *curriculum vitae,* a research paper suitable for presentation at a workshop, and evidence of teaching ability to the contact person indicated below. Three letters of reference will also be required before an offer of employment can be considered. Contact address: Jamie Pratt, Chair, Department of Accounting, KPMG Professor of Accounting, Kelley School of Business, Indiana University, 1309 E. 10th Street, Bloomington, IN 47405-1701; Phone: (812) 855-2657; Email: jpratt@indiana.edu. Indiana University is an Equal Opportunity/Affirmative Action Employer. Minorities, women, and persons with disabilities are encouraged to apply.

107 STANFORD UNIVERSITY, Graduate School of Business, invites applications for tenured and nontenured faculty positions in Accounting effective September 2004. We invite applications from candidates at all applicable stages of their careers who are interested in Financial Accounting, Managerial Accounting, Tax, and other areas. Candidates must have a Ph.D. and the highest level of accomplishment or promise in both scholarly research and M.B.A. and Ph.D. teaching. Applications should include *curriculum vitae* and copies of research publications. For efficiency, accuracy, and speed, applicants are strongly encouraged to submit applications electronically at http://www.gsb.stanford.edu/recruiting and upload *curriculum vitae,* publications, and teaching evaluations (if applicable) to that site. Alternatively, you may email electronic materials to Faculty_Recruiter@GSB.Stanford.Edu or postal mail (nonreturnable) materials in soft or hard copy to Faculty Recruiting Manager, Graduate School of Business, Stanford University, Stanford, CA 94305-5015; please allow 3 weeks processing. Three letters of recommendation should be sent to the preceding address. Deadline is January 30, 2004. Stanford University is an Equal Opportunity, Affirmative Action Employer.

108 RICE UNIVERSITY, Jones Graduate School of Management (JGSM), invites applications for tenure-track positions in Accounting at the Assistant Professor level commencing Fall 2004. Candidates with research interests in Empirical Financial Accounting or Empirical Managerial Accounting are preferred. Candidates for Assistant Professor should hold a Ph.D. or be nearing completion of their degree, and should demonstrate strong research potential and evidence of quality teaching. Entry-level faculty are assigned two semester-course equivalents per year for the first three years. Subsequently, the teaching load for all faculty is three semester-course equivalents per year. Applicants should send their *curriculum vitae,* three letters of recommendation, copies of recent working papers or dissertation proposal to Ms. Jackie Gambrell; Jones Graduate School of Management, Rice University, PO Box 2932, Houston, TX 77252-2932. Rice University is an Equal Opportunity/Affirmative Action Employer and actively encourages applications and nominations of qualified women and underrepresented groups.

109 HONG KONG SHUE YAN COLLEGE invites applications for an Assistant, Associate, or Full Professor position in Accounting tenable from February 1, 2004. Applicants should have a Ph.D. in Accounting or related area, a research record, relevant teaching experience, and the ability to teach Accounting Theory, Accounting Research Methodology, and/or International Accounting in our accredited honors degree program. Senior academics are encouraged to apply as there is no rigid retirement policy. Applications for visiting appointments (one semester or one year) are also invited. Total remuneration package may include both salary and furnished accommodation. For all applications, please send a letter of interest, complete *curriculum vitae*, contact details of three references, and salary expectations to the College Secretary, Shue Yan College, Braemar Hill, Hong Kong. Further information may be obtained from the College website: http://www.hksyc.edu; inquiries may be directed to info@hksyc.edu. Applications will be considered until the positions are filled.

110 SAN DIEGO STATE UNIVERSITY (SDSU), School of Accountancy, invites applications for an Assistant Professor position in Financial Accounting. Applicants should have, by August 2004, a Ph.D. or D.B.A. in Accounting or Business Administration with an emphasis in Accounting. Training and/or interest in International/Global Accounting is desired. The position is available August 2004. Applicant consideration begins December 2003. Compensation is dependent upon qualifications and is subject to final budget. If interested, email or send a letter stating an interest, a *curriculum vitae,* three recommendation letters, and examples of teaching and scholarship to G. E. Whittenburg, Acting Director (or email to gwhitten@mail.sdsu.edu), School of Accountancy, San Diego State University, San Diego, CA 92182-8221. SDSU is an Equal Opportunity Employer.

111 SUNY College at Brockport seeks applicants for a tenure-track Accounting Assistant Professor position in the Department of Business Administration and Economics, starting in Fall 2004. Candidates are required to have a Ph.D. or D.B.A. in Accounting. Scholarly potential demonstrated by publications or high-quality work in progress and research plan are needed. SUNY Brockport offers undergraduate programs in Accounting, Business Administration, and International Business. All programs are accredited by AACSB International. The College is located in a large, dynamic business community, near the Lake Ontario shoreline. See our website for full vacancy announcement: http://www.brockport.edu:81/~hr/Acct%20Vacancy%200803.doc. Send letter of application, *curriculum vitae,* teaching evaluations, graduate transcripts, and three references to Ms. Adrienne Collier, Affirmative Action Officer, 421 Allen Administration Building, 350 New Campus Drive, SUNY College at Brockport, Brockport, NY 14420-2929. Informal inquiries may be addressed to Dr. Sandeep Singh; Phone: (585) 395-5519; Email: ssingh@brockport.edu.

112 LOUISIANA STATE UNIVERSITY, E. J. Ourso College of Business Administration, Department of Accounting, invites applications and nominations for a Chair and Professor position. Required qualifications include an earned doctoral degree in Accounting or a related field; an established instructional and research record; administrative experience; active association with professional organizations; ability and willingness to interact with the Accounting profession and the overall business community; possess a record of scholarship commensurate with appointment as a tenured Professor of Accounting. For more information, see our website at http://www.lsu.edu. Review of applications will begin January 15, 2004 and will continue until a candidate is selected. Applications, resumes (including email address), and nominations should be sent to Search Committee Chair, Department of Accounting, E. J. Ourso College of Business Administration, Louisiana State University, Ref: Log #0195, Baton Rouge, LA 70803. LSU is an Equal Opportunity /Equal Access Employer.

113 FRANCIS MARION UNIVERSITY invites applications for a tenure-track Assistant/Associate/ Professor of Accounting position. Primary teaching area required: Financial Accounting; other teaching area: Accounting Information Systems. Demonstrated history of continuous engagement in scholarly activities is required. Advisement of students and other service activities are expectations. Qualifications include an earned doctorate in Accounting from AACSB-accredited school. ABD candidate with definite completion date and scholarly growth potential will be considered. Professional certification and experience are desired. Preference will be given to persons who have demonstrated a history of excellence in teaching and scholarly activities. The following Application Packet materials are needed: letter of interest (referencing #04-02); *curriculum vitae;* copies of all transcripts (official transcripts required of successful candidate); FMU Faculty Application; and names, addresses, and telephone numbers of three references. Screening of position will begin immediately. Position begins August 2004. For more information, please visit our website: http://www.fmarion.edu. Send Application Packet to Dr. Robert T. Barrett, Chair of Screening Committee, Francis Marion University, PO Box 100547, Florence, SC 29501-0547. Minorities and women are strongly encouraged to apply.

114 UNIVERSITY OF ST. THOMAS, College of Business, invites applicants for a tenure-track position in Accounting to begin Fall 2004, pending administrative approval. We prefer expertise in Taxation or Information Systems. We also value those with multiple teaching interests and abilities, including undergraduate- and graduate-level Financial or Managerial Accounting courses. Qualifications include Ph.D. (ABD considered with completion by Summer 2004), D.B.A., LLM (in Taxation)/CPA, or J.D. with Accounting master's degree; previous teaching and business experience preferred; demonstrated capability for research and commitment to excellence in college teaching. Submit cover letter (refer to position #200180) and *curriculum vitae* electronically at: http//:www.hr.stthomas.edu, or mail to HR, AQU217, 2115 Summit Avenue, St. Paul, MN 55105. The University of St. Thomas is an Affirmative Action and Equal Opportunity Employer.

115 DARTMOUTH COLLEGE, the Tuck School of Business, Graduate School of Management, is conducting a faculty search in Accounting. We are open to candidates interested in either Financial or Managerial Accounting, and we are open to all levels of appointment—Assistant Professor, Associate Professor, or Full Professor. However, there is a preference for those who have at least three years' experience since completing their Ph.D. We are interested in candidates who will produce research of excellent quality that will have high impact on the field. There must be evidence that the candidate can conduct research and teach at a level of quality consistent with Tuck's high standards. Send resumes and recommendations to Senior Associate Dean Robert G. Hansen, 100 Tuck Hall, Hanover, NH 03755. Dartmouth College is an EO/AA Employer. Women and minorities are encouraged to apply.

116 WINONA STATE UNIVERSITY, Winona, MN, invites applications for an Accounting tenure-track Assistant/Associate Professor position starting Fall 2004. Salary will be commensurate with experience. Minimum qualifications include Ph.D. or D.B.A., with a concentration in Accounting from AACSB-accredited school, completed by August 23, 2004. For job description, please see: http://www.winona.edu/humanresources; email rdelong@winona.edu; or call (507) 457-5639. Application review begins 11/01/03; position pending budgetary approval. AA/EOE.

117 THE UNIVERSITY OF TEXAS AT DALLAS (UTD) invites applications for four positions in Accounting and Information Management effective September 2004. Appointment at Assistant, Associate, or Full Professor level will be considered. Appointees must have (or expect) a Ph.D. or equivalent and demonstrate the ability to conduct a nationally recognized research program. Graduate education in a university emphasizing research training and ability, and interest in interacting with faculty colleagues on research projects in related disciplines are desirable. Applicants should send a complete *curriculum vitae* (educational history; teaching, research and work experience; list of publications, current research papers, and presentations), and names and addresses of three references to Academic Search #4103, The University of Texas at Dallas, PO Box 830688, M/S AD 23, Richardson, TX 75083-0688. For more information, visit http://www.utdallas.edu/dept/aim. UTD is an AA/EOE.

118 MIDDLE TENNESSEE STATE UNIVERSITY, Department of Accounting, invites applications for one tenure-track faculty position at the Assistant/Associate Professor rank, beginning August 2004. Candidates should possess an earned doctorate in Accounting. Applicants should have a strong commitment to excellence in teaching, research and publication, and service. MTSU has over 21,000 students, and is in Murfreesboro, 30 miles from Nashville. The Jennings A. Jones College of Business has more than 115 full-time faculty serving nearly 4,000 undergraduate and graduate students. The Department of Accounting offers an undergraduate degree in Accounting, an M.S. degree in Accounting/Information Systems, and supports the accounting concentration in the M.B.A. program. Review of applications begins November 1, and will continue until position is filled. Submit application letter, resume, transcripts, and three letters of reference to Dr. G. Robert Smith, Jr., Search Committee Chair, MTSU Box 50, Murfreesboro, TN 37132. Women and minorities are highly encouraged to apply.

119 CHEUNG KONG GRADUATE SCHOOL OF BUSINESS, Beijing, China, invites applications for a tenure-track position in the field of Accounting. The position can be at any level, but priority will be given to the level of Associate or Full Professor. Appointment will be from September 2003 or sooner. Applicant must have a Ph.D., and potential for excellence in research and teaching. For a senior appointment, application must have a demonstrated exceptional research and/or teaching record. We offer a competitive compensation package. Applicants should submit their *curriculum vitae,* any evidence supporting their teaching and research records, and ensure that three letters of recommendation are sent to Academic Associate Dean, Cheung Kong Graduate School of Business, Oriental Plaza, Tower E3, 1 East Chang An Avenue, Beijing 100738 P.R. China; Email: Recruit@ckgsb.com.

120 UNIVERSITY OF PITTSBURGH, Joseph M. Katz Graduate School of Business, invites applications for an anticipated tenure-stream position in the Accounting area at the rank of Assistant Professor beginning Fall 2004. Applicants should have a strong commitment to excellence in scholarly research and teaching. All areas of Accounting are appropriate. Candidates should send a recent *curriculum vitae* and statement of interest to Professor Nandu J. Nagarajan, Katz Graduate School of Business, University of Pittsburgh, Pittsburgh, PA 15260; email: nagaraja@katz.pitt.edu. We will begin reviewing applications on January 15 and continue to do so until the position is filled. The University is an Affirmative Action Equal Opportunity Employer.

121 GETTYSBURG COLLEGE invites applications for a two-year position in Accounting/Finance at the Assistant Professor level beginning August 2004. Candidates should have a Ph.D. or be near its completion and demonstrate strong teaching qualifications. The successful candidate's primary responsibility, will be to teach Introductory Financial Accounting. Visit http://www.gettysburg.edu for information about the college. Send a letter of application, academic resume, and three references to Dr. Karen Frey, Chair, Department of Management, Box 395, Gettysburg College, Gettysburg, PA 17325 or email: kfrey@gettysburg.edu. Applications arriving before December 15, 2003 will be assured of full consideration. Gettysburg College celebrates diversity and especially invites applications from members of any group that has been historically underrepresented in the American academy. The College assures Equal Employment Opportunity and prohibits discrimination on the basis of race, color, national origin, gender, religion, sexual orientation, age, and disability.

122 UNIVERSITY OF MIAMI, Department of Accounting, is seeking applicants at the Assistant Professor level for a tenure-track position beginning in 2004. Our primary areas of current teaching need are Auditing, Financial, and/or Tax. Our salary levels are competitive. The individual will be expected to carry a regular teaching load at both the undergraduate and graduate levels. Additionally, the individual will be expected to conduct and publish significant research in her/his area of interest—focusing particularly on important journals in the Accounting literature. Candidates should have a Ph.D. or equivalent. Please contact Dr. Oscar Holzmann, Chair, Faculty Recruiting Committee either by mail or email at Dr. Oscar Holzmann, Department of Accounting, 317 Jenkins Building, 5250 University Drive, University of Miami, Coral Gables, FL 33146; Email: holzmann@miami.edu. The University of Miami is an Equal Opportunity Employer.

123 THE UNIVERSITY OF TEXAS–PAN AMERICAN, Department of Accounting and Business Law, invites applications for tenure-track Accounting position at the Assistant level to begin Fall 2004. A doctorate in Accounting at time of employment is preferred, but ABDs in advanced dissertation stage may be considered. Candidates should have strong research and teaching skills, and the ability to work and communicate effectively in a culturally diverse environment. Salary is AACSB-competitive and commensurate with rank and experience. The College is AACSB-accredited and offers degrees at the B.A., M.B.A., and Ph.D. levels. It is located in beautiful, tropical south Texas. Review of applications begins immediately, and position will remain open until filled. Interested candidates should send *curriculum vitae* and a letter of interest to Dr. Ray Landry, Search Committee Chair, Department of Accounting and Business Law, College of Business Administration, The University of Texas–Pan American, 1201 W. University Drive, Edinburg, TX 78539. The University of Texas–Pan American is an Affirmative Action/Equal Opportunity Employer. Women and minorities are encouraged to apply.

124 THE UNIVERSITY OF TEXAS–PAN AMERICAN, College of Business Administration, invites applications for Department Chair of Accounting and Business Law to begin Fall 2004. Salary is AACSB-competitive and commensurate with achievements and experience. Candidates should have an earned doctorate in Accounting, an established research record warranting appointment at the rank of Associate or Full Professor, the ability to work and communicate effectively in a collegial and participative culturally diverse environment, and a willingness to build relationships with area professionals. Review of applications begins immediately, and the position will remain open until filled. Interested candidates should apply to Dr. Ray Landry, Department of Accounting & Business Law, College of Business Administration, The University of Texas–Pan American, 1201 W. University Drive, Edinburg, TX 78539; Email: raylandry@panam.edu. The University of Texas–Pan American is an Affirmative Action/Equal Opportunity Employer. Women, minorities, and senior faculty are encouraged to apply.

125 SAMFORD UNIVERSITY invites applications for a tenure-track position in the School of Business beginning Fall 2004. Candidates must hold an earned Accounting doctorate (ABDs will be considered); professional certification and experience are desirable. Candidates must exhibit strong teaching skills with a commitment to scholarship and service. Primary teaching responsibility includes Accounting Principles and one or more of the following: AIS, Auditing, Cost/Managerial, or Financial. The School of Business is AACSB-accredited. Samford's Business degrees include a Bachelor's in Accounting, a Master's in Accountancy aimed at preparing students for certification and success in the profession, and an M.B.A. The successful applicant is expected to be an active member of a Christian church and to exemplify a lifestyle consistent with a Christian university. Please submit a *curriculum vitae* to Dr. James P. Reburn, Area Coordinator of Accounting, Samford University, School of Business, 800 Lakeshore Drive, Birmingham, AL 35229; Email: jpreburn@samford.edu.

126 UNIVERSITY OF MINNESOTA, Carlson School of Management, invites applications for tenure-track faculty positions at the rank of Assistant Professor in Accounting. We offer a stimulating research environment with outstanding synergies across all Accounting research paradigms. Salary and benefits are competitive. All applicants must possess a doctoral degree (or be close to completion), and must have a strong commitment to Accounting research and teaching. Review of applications begins January 15, 2004 and will continue until the position is filled. Send applications, including a *curriculum vitae,* research papers, and at least two letters of recommendation from established scholars to Chair, Search Committee, Department of Accounting, 3-122 Carlson School of Management, University of Minnesota, 321 19th Avenue South, Minneapolis, MN 55455. The University of Minnesota is an Equal Opportunity Educator and Employer.

127 UNIVERSITY OF MINNESOTA, Carlson School of Management, Department of Accounting, invites applications for the Carl Nelson Professor of Accounting. Appointment will be made at the rank of tenured Associate or Full Professor, depending upon qualifications and experience, and consistent with Carlson School and University policy. We are seeking a scholar who has achieved national recognition in the field of Empirical Capital Markets Accounting Research and is committed to excellence in Accounting teaching. Applicants must have an established record of publications in top-tier academic journals and demonstrated excellence in teaching. Review of applications begins August 15, 2003 and will continue until the position is filled. Applications must include a *curriculum vitae,* two samples of recent research papers, and evidence of teaching quality. Candidates must possess a doctoral degree. Applications should be sent to Professor Frank Gigler, Honeywell Chair, Department of Accounting, 3-122 Carlson School of Management, 321 19th Avenue South, Minneapolis, MN 55455. Initial consideration of all applications will be conducted confidentially. Finalists will be asked to provide references. The University of Minnesota is an Equal Opportunity Educator and Employer.

128 MIDWESTERN STATE UNIVERSITY, College of Business Administration, Department of Accounting, invites applications for a tenure-track position effective Fall 2004 for the Bridwell Professorship in Accounting. A doctorate in Accounting, a record of teaching excellence in Auditing and Financial Accounting, scholarly research and refereed publications, and familiarity with AACSB International accreditation are required. Duties include, but are not limited to, teaching 9–12 hours per semester, productive research, service to the university, the profession, and the community, and student advising. Appointment will be as Full Professor. Interested candidates should send a letter of application that speaks to this advertisement, a current *curriculum vitae,* unofficial transcripts, and a list of three references to Dr. Ralph Fritzsch, Chair, Department of Accounting, College of Business Administration, Midwestern State University, 3410 Taft, Wichita Falls, TX 76308; Email: ralph.fritzsch@mwsu.edu. For more information is available at: http://business.mwsu.edu/.

129 UNIVERSITY OF ARKANSAS AT LITTLE ROCK invites applications for a tenure-track position as Chair of the Department of Accounting effective January 1, 2004. Candidates must have an earned doctorate with a major in Accounting from an AACSB-accredited university and a CPA certificate. Academic experience and publication record must be sufficient to merit appointment as an Associate or Full Professor. Administrative experience is preferred. Submit credentials to Dr. Judyth Swingen, Associate Dean for Graduate Studies, College of Business, UALR, 2801 S. University Avenue, LR 72204; Email: jaswingen@ualr.edu. Include the names of at least three references and their contact information. Official transcripts will be required upon employment. University of Arkansas at Little Rock is an AA/EO Employer. All applications are subject to disclosure. Persons hired must have proof of legal authority to work in the United States.

130 UNIVERSITY OF MARYLAND seeks applicants for a continuing nontenure-track teaching position in Accounting. The position is for nine months beginning August 2004, and is renewable subject to performance. Off-term teaching is available. Candidates must possess a doctorate and an excellent teaching record, especially at the Master's level. Teaching breadth will be valued. Certain student support and service responsibilities will be expected. Send a *curriculum vitae,* three letters of recommendation, and evidence of teaching effectiveness to Professor Oliver Kim, The Robert H. Smith School of Business, University of Maryland, College Park, MD 20742-1815. The University of Maryland is an Equal Opportunity/Affirmative Action Employer. Women and minorities are especially encourages to apply.

131 STATE UNIVERSITY OF NEW YORK (SUNY) AT BINGHAMTON, AACSB's accredited School of Management, invites applications for one Assistant Professor in Accounting for Fall 2004. Position is subject to budgetary approval. Preference will be given to applicants with teaching and research interests in Auditing, but all interested candidates are invited to apply. Candidates must have (or expect) a Ph.D. degree and demonstrate the ability to conduct a nationally recognized research program. Quality research is supported with a two-course/semester teaching assignment. Located three hours from New York City and one hour from Syracuse and Ithaca, Binghamton is a low cost-of-living regional medical/retail hub for 300,000 people. Interested candidates should send a *curriculum vitae* (preferably electronic) to Professor Sara Reiter, School of Management, Binghamton University, PO Box 6000, Binghamton, NY 13902-6000; Fax: (607) 777-4422; Email: sreiter@binghamton.edu. SUNY at Binghamton is an Equal Opportunity/Affirmative Action Employer.

132 THE UNIVERSITY OF LOUISIANA AT MONROE, Department of Accounting, invites applications (with primary interest in Auditing) for a tenure-track position for Fall 2004. The Accounting Program and the College of Business Administration are both accredited by the AACSB. Candidates must have an earned doctorate (or be ABD nearing completion); professional certification and experience are desirable. Salary and rank will be competitive and based on qualifications. Applicants should provide evidence of their commitment to excellence in teaching and scholarship, *curriculum vitae,* photocopies of undergraduate and graduate transcripts, and names of three references. Applications will be considered until the position is filled and should be directed to Dr. Michelle McEacharn, Head, Department of Accounting, The University of Louisiana at Monroe, 700 University Avenue, Monroe, Louisiana 71209-0110. For more information, visit the ULM website: http://www.ulm.edu. The University is an Equal Opportunity/Affirmative Action Employer located in a metropolitan area of 150,000 persons.

133 ARKANSAS STATE UNIVERSITY invites applicants for tenure-track Assistant/Associate Professor of Accounting beginning August 2004. Preferred candidates will have a Ph.D./D.B.A. in Accounting from an AACSB-accredited school. Professional certification and experience are also desirable. ABD near completion also will be considered. Candidate must possess a commitment to research and service compatible with our mission. Desired specialties include Financial, Managerial, and AIS. Send letter of application, current *curriculum vitae,* and names of three references to Dr. Tina Quinn, Search Committee Chair, Department of Accounting, Finance and Law, Arkansas State University, State University, AR 72467. Arkansas State University is an Equal Opportunity, Affirmative Action Employer and actively seeks the candidacy of minorities, women and persons with disabilities.

134 UNIVERSITY OF ILLINOIS AT CHICAGO invites applications for tenure-track positions at Assistant or Associate Professor levels from candidates interested in all areas of Accounting, especially Auditing, beginning August 2004. Applicants should possess a Ph.D. and exhibit capacity to produce quality research and commitment to teaching excellence. Visit http://accounting.cba.uic.edu/ for further information. UIC is one of 70 Research I universities in the country. The Department of Accounting is AACSB-accredited with 500 undergraduate and 100 graduate students, and the College of Business Administration has 2,500 undergraduate and 1,000 graduate students. Please submit *curriculum vitae,* representative publications, and three letters of references to Ram T. S. Ramakrishnan, CBA Accounting Dept, MC 006, 601 S. Morgan St., Chicago, IL 60607; Email: rramakri@uic.edu by February 28, 2004. UIC is an AA/EOE.

135 UNIVERSITY OF UTAH, School of Accounting and Information Systems, is recruiting for a new tenure-track faculty member in Accounting at an Assistant/Associate rank in any area of specialization, beginning in the 2004–2005 academic year. Applicants should possess a doctorate or be close to completion. Rank will be considered subject to qualifications. Salaries, research support, and teaching loads are competitive and fringe benefits are excellent. The School of Accounting and Information Systems and the David Eccles School of Business are both accredited at the undergraduate and graduate levels by the AACSB. Bachelor's, M.B.A., Master's of Accounting, and Ph.D. programs are offered. The application deadline is January 15, 2004 or until the position is filled. Please send application including dissertation proposal or working paper, *curriculum vitae,* and three letters of reference to David Plumlee, Director, School of Accounting and Information Systems, 1645 E. Campus Center Drive, Room 108, University of Utah, Salt Lake City, Utah 84112. The University of Utah is an Equal Opportunity, Affirmative Action Employer, encourages applications from women and minorities, and provides reasonable accommodation to the known disabilities of applicants and employees.

136 UNIVERSITY OF MONTANA is seeking to fill a tenure-track position at the Assistant Professor level beginning Fall Semester 2004. The primary teaching area is expected to be Financial Accounting. An appropriate Ph.D. must be completed prior to starting employment. Professional certification is desirable. UM Accounting programs hold separate accreditation from AACSB-International. Review of applications will begin immediately and position will remain open until filled. Applicants should send a cover letter, resume, and list of three references to Stanley Earl Jenne, Chair, Department of Accounting and Finance, University of Montana, Missoula, MT 59812-6808. Please direct questions to Professor Jenne at (406) 243-5872; Email: stan.jenne@business.umt.edu. Additional information is available at: http://www.business.umt.edu. UM is an AA/EO/ADA/State Vets Pref Employer. Criminal Background checks required of finalists.

137 MORGAN STATE UNIVERSITY, the Graves School of Business and Management, invites applications for a tenure-track position at the Associate Professor or Assistant Professor rank starting August 2004. Candidate should have a Ph.D. degree in Accounting from an AACSB-accredited institution, and research and teaching interests in Financial or Managerial Accounting. Candidates for Associate Professor should have a significant record of publications in recognized Accounting journals. Candidates for Assistant Professor should have demonstrated potential for Accounting research. Professional certification is desirable but not required. Salaries are AACSB-competitive. The University has Business School and Accounting accreditation by the AACSB. The Business School offers a B.S. degree in Accounting and a Ph.D. degree in Business Administration/Accounting. Master's level Accounting courses are offered in the general M.B.A. program. Interested candidates should send letter of interest and academic *curriculum vitae* to Sharon Gary Finney, Chairperson, Department of Accounting and Finance, School of Business and Management, Morgan State University, Baltimore, MD 21251, or email same to: sfinney@moac.morgan.edu. Morgan State University is an Affirmative Action/Equal Opportunity Employer located in Baltimore, Maryland.

138 COLUMBIA UNIVERSITY BUSINESS SCHOOL seeks applications for faculty positions in Accounting. Rank is open. Candidate with an interest in Financial Accounting will be preferred. Qualified applicants will possess a doctoral degree (or be close to completion) and evidence a strong commitment to both teaching and research. We offer a highly supportive research environment and easy interaction with faculty in other disciplines. Salary and benefits are competitive. Please send resume and a research paper to Professor Nahum Melumad, Columbia University Business School, 610 Uris, 3022 Broadway, New York, NY 10027. Applications should be received by the end of January 2004. An Equal Opportunity/Affirmative Action employer, women and minorities are especially encouraged to apply.

139 THE GEORGE WASHINGTON UNIVERSITY, School of Business and Public Management, Department of Accountancy, invites applications for one tenure-track and one nontenure-track/visiting position at the Assistant Professor level, with possibility of appointment as Associate Professor for exceptional candidates. Anticipated starting date is Fall 2004. A relevant doctoral degree is necessary for the tenure-track position. Strong ABD candidates will be considered if degree requirements can be completed prior to appointment. Potential or demonstrated commitment to scholarly research and teaching excellence is expected. For the nontenure-track/visiting position, a relevant doctoral degree is preferred. The successful candidate should have a strong commitment to teaching excellence, or a distinguished professional career. Send application, a current resume, recommendation letters, and current working papers to Professor Keith Smith, Chair, Department of Accountancy, SBPM, The George Washington University, Washington, D.C. 20052. Application review began on November 24, 2003 and will continue until positions are filled. The George Washington University is an Equal Opportunity, Affirmative Action Employer.

140 TULANE UNIVERSITY, A.B. Freeman School of Business, invites applicants for Assistant/Associate Professor of Accounting for both tenure-track and visiting positions. All candidates should be committed to excellence in teaching, and the tenure-track candidates should be committed to excellence in scholarly research as well. Salary is competitive and research support is excellent. Please send a *curriculum vitae* and an example of scholarly work to Professor Chi-Wen Jevons Lee, J.R. Cohen Chair in Business Administration and Associate Dean, A.B. Freeman School of Business, Tulane University, New Orleans, LA 70118-5669. Tulane University is an Equal Opportunity, Affirmative Action Employer.

141 SUNY AT BUFFALO, School of Management, invites applications for an Accounting Instructor starting Fall 2004, subject to budgetary approval. This is a full-time, nontenure-track position with renewable contracts. Teaching load is four sections (12 credit hours) per semester along with an expectation of some departmental service. M.B.A. or other applicable Master's degree or Ph.D. is required; CPA preferred. Candidates should have teaching experience in Accounting Systems and other Accounting areas, with tangible evidence of teaching excellence. Salary and fringe benefits are competitive. Send *curriculum vitae,* two letters of recommendation, and teaching evaluations to Professor Samuel Tiras, Recruiting Chair, Department of Accounting and Law, Jacobs Management Center, State University of New York at Buffalo, Buffalo, NY 14260-4000; Fax: (716) 645-3823; Email: stiras@buffalo.edu. SUNY at Buffalo is an Affirmative Action/Equal Opportunity Employer.

142 SUNY AT BUFFALO, School of Management, invites applications for one or more expected accounting faculty openings starting Fall 2004, subject to budgetary approval. Rank is open, but we are especially interested in applicants in the Auditing, Financial, Managerial, and Systems areas. Applicants must demonstrate capacity to undertake scholarly research publishable in leading journals and to provide quality teaching at the undergraduate, M.B.A., and/or Ph.D. level. Doctorate in hand (at the Assistant level) or near completion from a reputable research university is expected. A substantial research record is a requirement for senior positions. Salary and fringe benefits are competitive. Send *curriculum vitae,* two letters of recommendation, dissertation proposal, other research paper(s), and teaching evaluations to Professor Samuel Tiras, Recruiting Chair, Department of Accounting and Law, Jacobs Management Center, SUNY at Buffalo, Buffalo, NY 14260-4000; Fax: (716) 645-3823; Email: stiras@buffalo.edu. SUNY at Buffalo is an Affirmative Action/Equal Opportunity Employer.

143 UNIVERSITY OF CONNECTICUT, School of Business, invites applications for the Head of the Department of Accounting. Qualified candidates must possess a record of scholarship commensurate with appointment as a tenured full Professor of Accounting. Candidates should have strong leadership and administrative skills and a demonstrated interest in teaching excellence and program development. The Department has 20 full-time faculty members (15 tenure-track and 5 in-residence) working with 340 undergraduate, 80 Master's, and 10 Doctoral students. Priority will be given to applications and nominations received by December 1, but applications will be considered until the position is filled. Send *curriculum vitae* and other supporting materials to Dr. Thomas O'Brien, Chair, Accounting Department Head Search Committee, University of Connecticut, 2100 Hillside Road, Storrs, Connecticut 06269-1041; Email: thomas.obrien@uconn.edu. See http://www.uconn.edu for information about UConn. The University of Connecticut is an Affirmative Action/Equal Opportunity Employer. (Search # 04A201)

144 ANGELO STATE UNIVERSITY invites applications for a tenure-track position at the Assistant Professor level beginning in Fall 2004. Duties include teaching undergraduate and graduate courses in Accounting, advising students and assisting with departmental activities and programs, conducting research leading to publication, and participating in various service-related activities. A Ph.D. in Accounting preferred. Applicants with non-Accounting doctorates must have 18 hours graduate work in Accounting. Applicants should have the ability to communicate effectively with students and be dedicated to excellence in teaching. Please submit a cover letter, *curriculum vitae*, and ASU application, which is available at: http://www.angelo.edu/forms/pdf/appfacappt-web.pdf. Review of applications will begin January 15, 2004 and continue until the position is filled. Submit applications to Thomas A. Bankston, Department of Accounting, Economics, and Finance, ASU Station #10908, San Angelo, TX 76909-0908.

145 MIDDLE TENNESSEE STATE UNIVERSITY, Jennings A. Jones College of Business, is accepting applications for the Chair, Department of Accounting (Position #110130). The Chair is responsible for the academic leadership of the department, attracting and retaining Accounting majors, working with other business school programs, faculty recruitment, and maintaining and enhancing the connection between the department and the Accounting profession. A major goal of the department is to achieve AACSB accounting accreditation. Visitation will occur this academic year. The Chair reports to the Dean of the College. An applicant must have an earned doctorate degree in Accounting or a related field, an established instructional and research record, demonstrated involvement with quality student services, and active association with professional organizations. Rank and salary are commensurate with education and experience. Review of applications begins October 20; position is for Fall 2004. For more information contact Professor Stan Gambill, Chair, Search Committee; Phone: (615) 898-2749; Email: sgambill@mtsu.edu.

146 ADELPHI UNIVERSITY, School of Business, invites applications for two tenure-track positions: Assistant/Associate Professor of Accounting. Adelphi University, School of Business, has an exciting agenda of change and growth. The positions require an earned doctorate in Accounting, a record of scholarly publications, and a solid teaching record. Professional experience/certification is a plus. We are seeking one candidate with teaching interests in AIS and Auditing, or in Advanced Accounting and Not-for-Profit. A unique niche exists for an individual interested in teaching several sections of an M.B.A. course in Shareholder Value Creation. Both candidates should be comfortable with teaching introductory graduate courses in Financial or Managerial Accounting. Start date is Fall 2004. Applications will be accepted until the positions are filled. Please submit your *curriculum vitae*, including references, to Grace M. Conway, Chair Department of Accounting and Law, School of Business, Hagedorn Hall, Adelphi University, One South Avenue, Garden City, New York 11530. Adelphi University is an Equal Opportunity/Affirmative Action Employer.

147 EASTERN MICHIGAN UNIVERSITY, Department of Accounting and Finance, seeks applicants for two (2) tenure-track Assistant Professor positions for Fall 2004. The first position requires a primary interest in Managerial Accounting and the other position requires a primary interest in Accounting Information Systems. Flexibility regarding instructional area is necessary along with appointment in the primary areas. A Ph.D. or D.B.A. with a major in Accounting from an AACSB-accredited school or its equivalent is preferred for Assistant Professor positions. ABD with substantial evidence of potential for excellence in teaching and research will be considered for an Assistant Professor position. CPA or other professional certifications and teaching experience are desired. Other duties include maintaining strong ties to the professional accounting community, developing curriculum, and advising students. Salary is competitive. EMU is a public, comprehensive, metropolitan university of 25,000 students in southeastern Michigan, and its College of Business is AACSB-accredited. Submit letter, *curriculum vitae,* transcripts, teaching evaluations, and three reference letters to Posting# F0432, Eastern Michigan University, Academic Human Resources, 202 Boone Hall, Ypsilanti, MI 48197. Review of applications will start December 1, 2003 and continue until the positions are filled. Contact Dr. Susan Kattelus by email at susan.kattelus@emich.edu for more information and/or view the department's website at: http://www.accfin.emich.edu. Women and members of minority groups are encouraged to apply. EMU is an Affirmative Action/Equal Opportunity Employer committed to faculty, staff, and student diversity.

148 UNIVERSITY OF MASSACHUSETTS AMHERST, Department of Accounting and Information Systems, invites applications for a tenure-track position as Assistant/Associate Professor of Accounting. All research areas will be considered but preference will be given to behavioral accounting research; we also have a preference for those teaching in the Financial and Auditing areas. Start date is September 2004. All candidates should have a doctoral degree and a strong commitment to scholarly research and excellence in teaching. Salary is competitive. Please send *curriculum vitae,* names and addresses of at least three references, and any research papers or publications to Professor Thomas Kida, Chair, Search Committee, Department of Accounting and Information Systems, Isenberg School of Management, University of Massachusetts Amherst, 121 Presidents Drive, Amherst, MA 01003. Deadline for applications is February 15, 2004. Applications will be accepted until a successful candidate is found. The University of Massachusetts Amherst is an Affirmative Action/Equal Opportunity Employer. Women and members of minority groups are encouraged to apply.

149 MANHATTAN COLLEGE, School of Business, seeks applicants for a tenure-track position as an Assistant or Associate Professor of Accounting, beginning in August 2004. Candidates must have a Ph.D. or D.B.A. in Accounting as well as evidence of teaching effectiveness and research competence. Responsibilities include teaching undergraduate courses in Accounting, engaging in scholarly activities, and participating in campus activities. The Department seeks a candidate willing to teach Taxation, but all qualified candidates are encouraged to apply. Manhattan College is an independent Catholic college founded by the De La Salle Christian Brothers. The College is in the Riverdale section of New York City. Recruiting begins immediately and continues until the position is filled. Salary and rank will be based on qualifications. Please send a letter of application, *curriculum vitae,* and list of three references to Dr. Mary Michel, Chairperson of Accounting, Law and Computer Information Systems, School of Business, Manhattan College, Riverdale, NY 10471; Email: mary.michel@manhattan.edu. Manhattan College is an Affirmative Action/Equal Opportunity Employer.

150 GEORGE MASON UNIVERSITY, School of Management, invites applications for Accounting faculty tenure-line positions at all ranks. Preference will be given to candidates with teaching and research interests in Accounting issues relevant to strategy/policy, including corporate governance; entrepreneurship; or international business. Successful candidates will have a Ph.D. and a strong commitment to excellence in teaching and scholarly research and will also be considered for appointment as the CPA Professor of Public Accounting. Located near the nation's capital in Northern Virginia, the School of Management and Accounting Program are accredited by AACSB International and offer an undergraduate degree in Accounting and graduate degrees in M.B.A., E.M.B.A., Technology Management, and Bioscience Management. Information about the School of Management is available at: http://www.som.gmu.edu. Send application letter, *curriculum vitae,* evidence of teaching effectiveness and scholarly research, and three references to Dr. David Harr, Senior Associate Dean, George Mason University, Mail Stop 1B1, 4400 University Drive, Fairfax VA 22030-4444; Email: dharr@gmu.edu. The University is an EEO/AA Employer.

151 OREGON STATE UNIVERSITY invites applicants for a tenure-track Assistant/Associate position in Auditing effective September 2004. Responsibilities include research, quality teaching, and professional activities in the field of Accounting. Candidates should hold the appropriate doctorate. Professional certification is desirable. Salary is competitive; fringe benefits are excellent. Oregon State University is located in Corvallis, rated the second-best small city in America. Interested applicants should submit a letter of interest, current *curriculum vitae,* recent teaching evaluations, evidence of scholarly work, and a list of three references to Dr. Roger Graham, College of Business, 200 Bexell Hall, Oregon State University, Corvallis, Oregon 97331-2603. For full consideration apply by March 15, 2004. Applications will be accepted until the position is filled. OSU is an Equal Opportunity Employer and seeks diversity.

152 MONTANA STATE UNIVERSITY–BILLINGS, College of Business, invites applications for a tenure-track Assistant/Associate/Full Professor position in Accounting to being Fall Semester (August) 2004. Responsibilities include teaching a combination of lower- and upper-division Accounting courses; interest in Financial Accounting preferred. Candidates should have demonstrated excellence in teaching, willingness to work with new technologies in the classroom and in distance delivery; also CPA/CMA, relevant work experience, and interest in working with the business community. Ph.D. or D.B.A. is required and candidate must be "academically qualified" to teach in area according to the AACSB criteria. Scholarly activity and publication are required. Position is contingent upon funding; salary subject to collective bargaining agreement. Preference review begins December 1, 2003; position open until filled. For additional information, please see http://www.msubillings.edu. Send a letter of application, current resume, and three recent letters of reference to Search #2004-02, Human Resources Office, MSU-Billings, 1500 University Drive, Billings, MT 59101-0298; Phone: (406) 657-2278; Fax: (406) 657-2120. ADA/AA/EEO.

153 CASE WESTERN RESERVE UNIVERSITY, Weatherhead School of Management, invites applications for Assistant Professor of Accounting. Required qualifications include a doctorate in Accounting and a strong record of potential scholarship. A proven record of quality teaching is very important. Although candidates with a specialization in any Accounting area are invited to apply, we have a preference to those in the Financial and Managerial areas. CWRU currently offers undergraduate and master's level degree programs including a B.S. in Accounting and a Master's of Accountancy. In addition, faculty are heavily involved in M.B.A. program teaching and the supervision of doctoral students. All programs are AACSB-accredited. Additional information can be found at http://www.weatherhead.case.edu. Send *curriculum vitae* and other materials to Tim Fogarty, Department of Accountancy, Weatherhead School of Management, Case Western Reserve University, Cleveland, OH 44106-7235; Email: timothy.fogarty@case.edu. CWRU is an Affirmative Action/Equal Opportunity Employer. Applications from women and minority candidates are especially welcome.

154 FAYETTEVILLE STATE UNIVERSITY, School of Business and Economics, Department of Accounting, is seeking two candidates for an Assistant Professor/Associate Professor. Candidates must have earned a Ph.D. in Accounting; CPAs, CMAs, or CIAs preferred, ABDs in their final dissertation stage will be considered. Candidates for the rank of Associate Professor must show evidence of scholarly and professional activities. Responsibilities for Position One will include Doctorate in Accounting with demonstrated ability to teach in the following areas: Financial, Managerial, and Cost Accounting and M.B.A. courses. Responsibilities for Position Two will include Doctorate in Accounting with demonstrated ability to teach in the following areas: Financial Accounting, Accounting Systems, Advanced Accounting, Auditing, and M.B.A. courses. Each faculty member is expected to teach 12 semester hours, engage in scholarly and professional activities, and participate in student advisement and university services. Positions will be filled for Fall 2004. Contact Ulysses Taylor, Fayetteville State University, School of Business and Economics, Room 382, Fayetteville, NC 28301; Telephone: (910) 672-1998; Email: utaylor@uncfsu.edu.

155 UNIVERSITY OF SOUTHERN CALIFORNIA, the Leventhal School of Accounting at the Marshall School of Business, invites applications for tenure-track positions in Accounting. We are considering applicants for untenured and tenured positions at all ranks. Minimum qualifications include a Ph.D. with an emphasis in Accounting, relevant experience in teaching at the college/university level, and evidence of research competence and scholarly publications. Review of applications will begin December 1, 2003 and continue until the positions are filled. Salary is dependent on qualifications and the benefits package is excellent. A complete packet of application will include a letter of interest, detailed *curriculum vitae,* three letters of reference, and presentation paper. Qualified candidates should send materials to Administrative Coordinator, University of Southern California, Leventhal School of Accounting, ACC 101D, MC 0441, Los Angeles, CA 90089-0441. USC is an AA/EEO Employer and encourages the application of women and minorities.

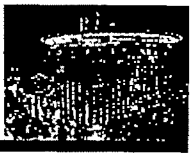
PMA

www.performanceportal.org

CONFERENCE

28-30 July 2004

Edinburgh International Conference Centre, UK



The 4th International Conference on Theory and Practice in Performance Measurement and Management

PUBLIC AND PRIVATE

CALL FOR PERFORMANCE PAPERS & PARTICIPATION

Keynote Speakers
Professor Sir Andrew Likierman, London Business School
Professor Chris Ittner, The Wharton School

Conference Chairs
Professor Andy Neely and Dr Mike Kennerley
Cranfield School of Management

Building on the tremendous success of the past conferences *'Performance Measurement and Management – Public and Private'* is the fourth in a series of bi-annual conferences focusing on the theory and practice of performance measurement and management. As previously, it will be a unique international forum for inter-disciplinary discussion of performance measurement research and concepts. No other event better facilitates dialogue in this field of performance measurement and provides the rare opportunity for discourse between academics and practitioners. Traditionally one third of presentations have been from practitioners and contributions have come form a broad mix of disciplines related to the topic of performance measurement and management, including measurement in the public sector, marketing performance, intangibles and intellectual capital, designing measurement systems, people performance, stakeholder measurement and measurement frameworks. Broad themes have been identified for the conference. Although these are not exclusive, submissions which strive to tackle these areas are encouraged. In addition to the above themes there will be a number of Streams running through the conference. More details on both the themes and streams are available on the conference website: www.performanceportal.org/pma2004.htm.

Themes:

- **The Impact of Performance Measurement & Management**
- **Performance Measurement & Culture**
- **Performance Measurement & Change**
- **Designing & Implementing Performance Measurement & Management Systems**
- **Managing Performance Measurement Systems**
- **Measuring Different Dimensions of Performance**
- **Measurement & Management of Intellectual Capital & Intangible Assets**
- **Performance Measurement Frameworks**
- **Disciplinary/Functional Perspectives on Performance**
- **Performance Measurement & Management in Difference Organisational contexts**
- **Performance Measurement & Management & other systems - such as Budgeting, Strategic Planning**

Additional Streams:

i) Critical Perspectives on PM
ii) Marketing Measurement
iii) Human Resources and Compensation
iv) Predictive Performance Measurement
v) Measuring Intellectual Capital and Intangibles
vi) AIM Research Fellows
vii) Corporate Social Responsibility
viii) PM & Management Control
ix) Measuring the Quality of Public Governance
x) BAM PM Special Interest Group

SUBMISSION DETAILS:

In order to engage both academics and practitioners here are two types of submissions – **Academic Research Submissions** and **Practical Application Submissions**. To submit an extended abstract for PMA 2002 please follow the guidelines set out in the full Call for Papers and email your submission by **16 January 2004**. The date by which full papers will be due is **14 May 2004**.

Conference Organised on behalf of the Performance Measurement Association by the Centre for Business Performance at Cranfield School of Management.

Cranfield
UNIVERSITY
School of Management

American University in Cairo

Founded in 1919, AUC's campus is located in Cairo, Egypt, and its degree programs are accredited by the Commission on Higher Education of the Middle States Association of Colleges and Schools. For more information see our website at www.aucegypt.edu. One- two- or three-year appointments subject to mutual agreement will begin September 2004. Renewal of an appointment depends upon institutional needs and/or the appointee's performance. The normal teaching load is three courses per semester and English is the language of instruction. Salary and rank are according to scale based on qualifications and professional experience. For expatriates, housing, annual round-trip air travel for appointee and qualifying dependents, plus schooling for up to two children are included. In view of AUC's protocol agreement with the Egyptian Government, which requires specific proportions of Egyptian, U.S., and third-country citizen faculty, at this time preference will be given to qualified applicants who are U.S. citizens.

ACCOUNTING – Department of Management

The successful candidate will be expected to teach graduate and undergraduate courses in accounting and participate in the preparation for departmental accreditation by the AACSB, and in professional growth and scholarly research. For appointments at assistant professor level, applicants should have Ph.D in hand, and demonstrate potential for a strong record in research and excellence in teaching. Teaching/industry experience, professional certification, and recent scholarly research and publication are preferred. We are seeking applicants who are willing to maintain AACSB qualifications in their appropriate field. All areas of specialization are considered. This position is potentially tenurable.

E-mail a letter of interest specifying position #MGMT-1 and attach a current C.V. and names and addresses of three references to facultyaffairs@aucnyo.edu or mail to:

Dr. Earl (Tim) Sullivan, Provost
The American University in Cairo
420 Fifth Avenue
Floor 3
New York, NY 10018-2729

For full consideration, candidates must also complete the Personnel Information Form provided at **http://forms.aucegypt.edu/ provost/pif3.html.**
Applications accepted until position is filled. Formal review of candidates to begin November 1, 2003.

The A.B. Freeman School of Business of Tulane University is seeking a distinguished scholar to fill the **Albert Harry Cohen Chair in Business Administration.** Candidates must have appropriate experience and research record for appointment at the chair level. The candidate is expected to teach in the areas of Financial or Management Accounting. To apply, please send a resume, representative research output, evidence of teaching capabilities, and at least three recommendation letters to: Prof. Chi-Wen Jevons Lee, J. R. Cohen Chair in Business Administration and Associate Dean, A. B. Freeman School of Business, Tulane University, New Orleans, LA 70118-5669, USA.

Tulane University

Tulane University is an Affirmative Action, Equal Opportunity Employer

AWARD WINNERS

Congratulations to John Core, Wayne R. Guay, and S. P. Kothari whose article, "The Economic Dilution of Employee Stock Options: Diluted EPS for Valuation and Financial Reporting" (*The Accounting Review* July 2002, pp. 627–652) has been chosen by the Financial Executive Research Foundation, Inc. as *The Accounting Review* article of the year 2002. This award is part of an initiative by the Stan Ross Department of Accountancy at Baruch College in conjunction with the American Accounting Association and the Financial Executive Research Foundation, Inc. (FERF) to recognize the contribution of academic research to financial reporting practices. The authors received a $5,000 cash award that was sponsored by the Stan Ross Department of Accountancy and presented at the Baruch/FERF academic conference on November 7, 2003.

THE CHINESE UNIVERSITY OF HONG KONG

ADVANCE AND EXCEL

School of Accountancy

The School of Accountancy is a pioneer and leader in accounting education in Hong Kong and Asia since 1963. The School has established an excellent reputation for professional education and research. It offers BBA, MAcc, MPAcc, MPhil and PhD degree programmes in accountancy. Its premier business and accounting programmes are keenly sought-after in Hong Kong. To further enhance its role as an international center for excellent accounting education and research, established and promising accounting scholars in all fields of accounting around the world are invited to join our strong and devoted faculty team.

Applications are now invited for:

Assistant Professors/Associate Professors/Professors or Visiting Scholars/Professors *(Ref. 03/078(020)/2)*

Applicants should have a PhD degree (those in the final stage of writing their dissertation may also apply). Appointees will be required to teach courses at both undergraduate and postgraduate levels, as well as in day-time and evening programmes. Commitment to teaching excellence, quality scholarly research and School/University service is required. Appointments will be made on fixed-term contract basis for two to three years. Visiting appointments of shorter duration may also be arranged. Details of the School can be found at *http://www.baf.cuhk.edu.hk/acy*.

Salary and Fringe Benefits: Starting salary and level of appointment will be commensurate with qualifications and experience. The University offers a competitive fringe benefit package including a contract-end gratuity.

Further information about the University and the general terms of service for teaching appointees is available at *http://www.cuhk.edu.hk/personnel*. The terms mentioned herein are for reference only and are subject to revision by the University.

Application Procedure: Please send full resume, copies of academic credentials, a publication list and/or abstracts of selected published papers, together with names, addresses and fax numbers/e-mail addresses of three referees to whom applicants' consent has been given for their providing references (unless otherwise specified), to Professor James Xie, Director of School of Accountancy, The Chinese University of Hong Kong, Shatin, N.T., Hong Kong or email to xie@baf.msmail.cuhk.edu.hk. Applications will be accepted until the positions are filled. The Personal Information Collection Statement will be provided upon request. Please quote the reference number and mark 'Application - Confidential' on cover.

Lingnan 嶺南大學
University

Lingnan University is the only liberal arts institution in the Hong Kong Special Administrative Region. It offers honours bachelor degrees and postgraduate programmes up to doctoral level. Applications are invited for the following posts in the Department of Accounting and Finance:

Professor / Associate Professor / Assistant Professor (Post Ref.: 03/72/AAA)

The appointee should be able to teach in at least three of the following areas: Financial Accounting, Managerial Accounting, Auditing, Taxation, Accounting Systems, Legal Aspects of Business, Financial Econometrics, Hong Kong Monetary and Financial Systems, Investment, Capital Markets and Securities Regulations. Applicants should have a PhD degree in Accounting or Finance or a closely related field, relevant experience and a good track record in research and scholarly activities.

Monthly Salary Ranges (currently under review):

Professor	HK$69,854 - HK$90,858
Associate Professor	HK$57,056 - HK$75,109
Assistant Professor	HK$41,916 - HK$66,096

(Approximate exchange rate US$1=HK$7.8)

Substantial fringe benefits will be provided. Applicants are invited to send in their dossier as soon as possible, together with a personal data sheet (Form R1, which is obtainable from *http://www.LN.edu.hk/main/jobs/form.htm*). The University reserves the right not to fill the posts or to fill the posts by invitation or by search. For more information on Lingnan University, please visit the university's website: *http://www.LN.edu.hk*.

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